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"THE BASIC STOCK OF ALL HUMANITY"?

Negritos of the Phillipine Islands. Dr. Bean thinks they are a fusion of the three fundamental races of mankind, the Australoid, Iberian and Primitive, the former predominating. This Australoid stock, he suggests, may be the original one from which the other two are derived, unless it is itself a result of the fusion of the Iberian and Primitive somewhere in eastern Asia in prehistoric times. The Australoid stock is that which is chiefly responsible for the American negro. (Frontispiece.)

(See "The Phillipine Negritos," page 216.)

CROSSING BISON AND CATTLE

First Cross Dangerous But Results are Better in Each Succeeding Generation—Hope of Taking Fur and Hump of Bison and Placing Them Upon Back of Domestic Ox.

MOSSOM M. BOYD, *Bobcaygeon, Ontario, Canada.*

WHEN in 1908 I gave an account[1] of my firm's (Mossom Boyd Co.) experiment up to that date in crossing the American bison with domestic cattle, we had just begun the third stage of the experiment; and although four years[2] in a work of this kind is too short a time in which to accumulate much data of interest, I will nevertheless report what little there is to tell.

The first stage of the experiment was the crossing of the two species.

The second consisted of crossing the hybrids thus produced with pure-bred animals of both species.

And the third stage, in which we are now engaged, consists of breeding from parents, both of which are of mixed blood.

The produce of the first stage in which both parents were pure, we call hybrid buffaloes or half-bred-buffaloes, or for short, hybrids. The produce of the second stage in which one parent was pure and the other of mixed blood we call three-quarter-buffaloes, or one-quarter-buffaloes, or one-eighth-buffaloes, as the case may be, reserving for the sake of distinction the term "cattalo" for the produce of the third stage in which both parents are of mixed blood; and we put before the word cattalo a figure to denote the proportion of bison blood; for example, one-half cattalo (containing 50% of bison blood), one-quarter cattalo (containing 25%), and so on. Breeders will readily understand that there is a radical difference between what we now call cattaloes and three-quarter, one-half, one-quarter and one-eighth-buffaloes. Excepting two or three, we have not been raising animals carrying less than one-eighth of buffalo blood.

CHARACTERS OF THE HYBRIDS.

In the first stage the hybrids, of which we succeeded in raising about 30, were all of one type; so uniformly alike that individuals of the same color were as hard to distinguish from one another as are Polled Angus cattle. Those from Hereford dams had white faces, but almost no other white; and excepting these, all were practically whole-colored, and were either brindled or black. The black was not the jet black of the Polled Angus but had a rich tinge of brown. The brindled varied between a tawny red and brown.

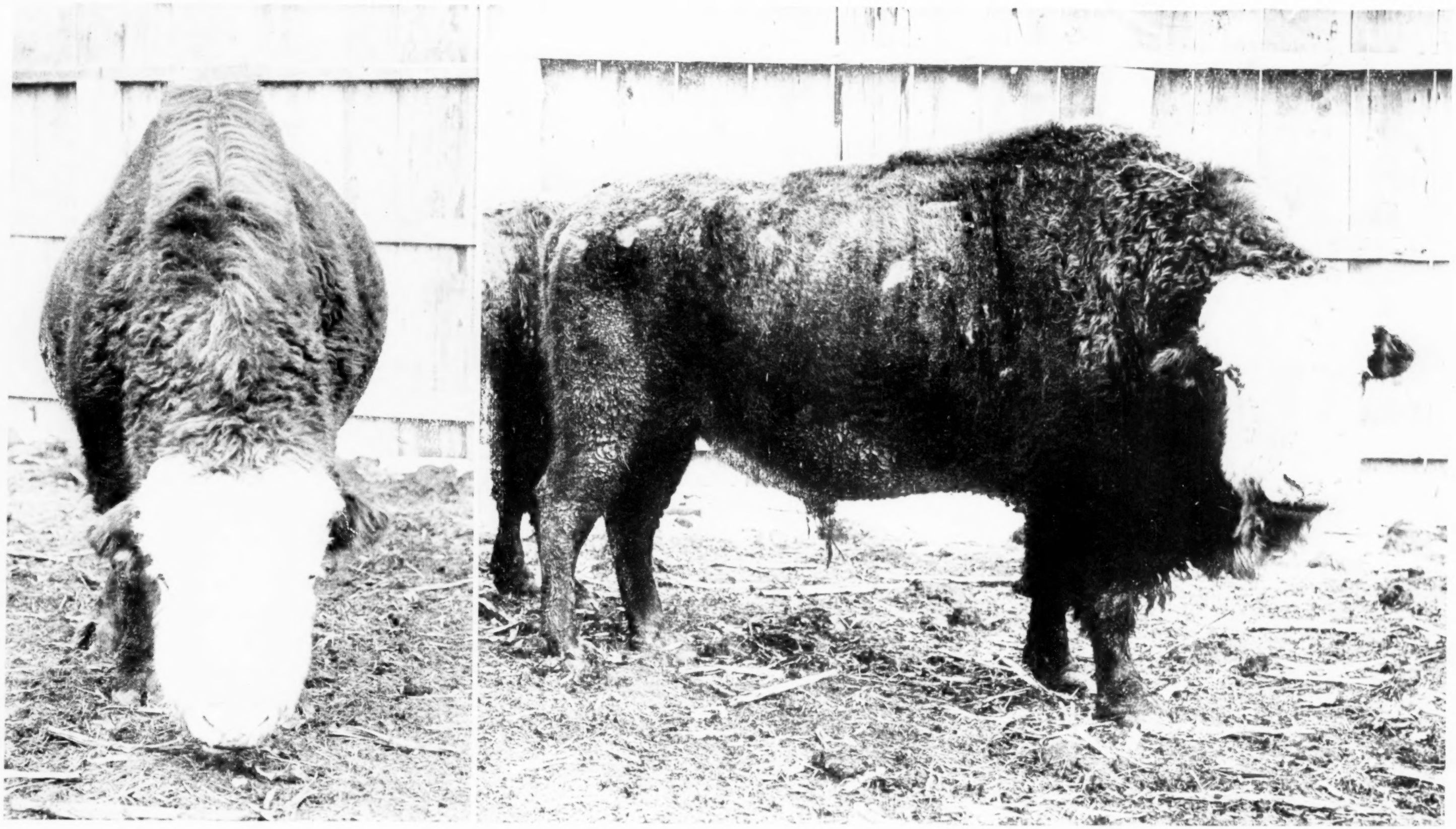
The hybrids were larger than the bison; of "smoother" build; wider chested, had better hind quarters, stood straighter on their hocks, and had coats of a better color, with more lustre and less tufted. Altogether they were much finer-looking animals than the bison.

The three-quarter-buffaloes, got by using a bison bull on the hybrid females, were likewise all of one type; as much like one another as are Polled Angus cattle; and they were about midway in appearance between their parents. The five we raised (one bull and four females) were in color very much like the bison, but of a somewhat darker shade. One, whose dam was half-Hereford, had the front of its face white.

The one-quarter-buffaloes, got by using Polled Angus and Hereford bulls on the hybrid females, were all of one

[1]A Short Account of an Experiment in Crossing the American Bison with Domestic Cattle, by Mossom M. Boyd. American Breeders' Association Annual Report, vol. IV, p. 324 ff.

[2]This paper was prepared in January, 1913. Mr. Boyd writes that at present his company has 31 head of grade buffaloes and cattaloes in its herd.



"QUINTO PORTO": FIVE-EIGHTHS BUFFALO AND THREE-EIGHTHS HEREFORD.

This striking result of hybridization of the American bison is out of a one-quarter-buffalo, three-quarter Hereford cow, by a pure buffalo bull. His exact pedigree is given in the text. This cross was particularly interesting, Mr. Boyd remarks, because it represented the nearest approach to a repetition of the dangerously severe first cross, when a pure Hereford cow was bred to a pure buffalo bull. Male calves are rarely, if ever, born from such a cross, but this example shows that the presence of one-fourth buffalo blood in the cow is sufficient to insure the possibility of producing a fertile bull calf. (Figure 1.)

A "HEREFORD" WHO IS FIVE-SIXTEENTHS BUFFALO.

"Huron," son of "Quinto Porto" (shown on preceding page), out of a pure Hereford cow. It is from such crosses as these or one generation further removed (say 10% to 15% buffalo blood) that it is hoped to improve the range cattle of the colder parts of the west, increasing their hardiness and improving their meat-producing and fur-producing qualities. (Figure 2.)

type in form, but varied in color, and likewise were about midway in appearance between *their* parents. The same remarks apply to the one-eighth-buffaloes, got from a second cross by the same breeds of domestic bulls.

An ordinary observer might mistake the three-quarter-buffaloes for bison; and he would scarcely distinguish the one-quarter-buffaloes from domestic cattle, except for the finer quality of hair. The one-eighth-buffaloes he would not distinguish at all from domestic cattle.

Accordingly, in stage one we got animals that were all of one type. In stage two, animals that were all of one type or all of another, according as we used the bison bull or the domestic bull; and the proportion of bison blood always determined the measure of likeness to the bison.

In stage three, as was expected, the proportion of bison blood no longer determines the likeness to the bison. We have two one-half-cattaloes, yearlings, quite as like the bison as are our three-quarter buffaloes; and among 12 cattalo calves of 1912 are a one-half-cattalo, a three-eighths-cattalo and a one-quarter-cattalo, which, notwithstanding their difference in grade with respect to bison blood, show equally the

A FIRST-GENERATION HYBRID.

This cow represents the most difficult achievement of hybridizing buffaloes—namely, the first cross, between a pure buffalo bull and an ordinary cow. (The reverse cross cannot be made.) The percentage of failures in this cross is extremely high, and almost no male calves are produced, partly, at least, because their height just back of the shoulders prevents them from passing through the cow's pelvis. Hybrid cows of the sort shown in the photograph are often barren, but fertility is gained at each subsequent cross back to cattle blood. (Figure 3.)

HEIFER WITH HALF BUFFALO BLOOD.

The hybrids average rather larger in size than either parents (buffalo cows weighing only 1,000 to 1,200 lbs., while the buffalo bulls weigh from 1,800 to 2,000). Their meat is said to be indistinguishable from beef of the same age and quality, but they produce more of it, and the cuts are considered choicer. The first hybrids of this sort were produced as far back as the revolutionary period; in 1908 W. T. Hornaday estimated that there were 260 of them in the United States, 57 in Canada and 28 in Europe, a total of 345, while the number of pure bison in the world at that time barely exceeded 2,000 (Figure 4.)

bison characteristics. The one-quarter-cattalo has, we think, somewhat the best coat of the three.

HOPE FOR THE FUTURE.

This is encouraging, especially in these early matings, and seems to warrant the expectation that in later generations, after there has been time for selection, there may be seen on cattaloes, having say 10% or less of bison blood, as good fur as is now seen on the one-half and three-quarter-buffaloes.

Moreover, there is a promising prospect of greatly improving the beef carcass also, for the bison carries an exceedingly high percentage of beef on his back, which is the most valuable part of a beef carcass; and the inheritance of this quality may well be encouraged by selection. The hump of the buffalo is not a mass of fat as some people suppose, but is formed by neural spines in length fully double those of domestic cattle, and by the huge muscles which lie alongside and fill up the angle between these neural spines and the ribs. In a rib roast of beef, these muscles constitute the upper cut, and I have had on my table a hybrid roast with an upper cut nine inches deep.

I give the measurements of the length (measured from the centre of the spinal cord) of the neural spines of the smallest of our three bison bulls. They are in inches respectively: 11, $16\frac{1}{2}$, $16\frac{1}{2}$, 16, $15\frac{1}{2}$, $14\frac{1}{4}$, 13, —, $10\frac{1}{2}$, $9\frac{1}{2}$, 8, —, $6\frac{3}{4}$, —, $4\frac{3}{4}$, —.

THREE-QUARTERS-BUFFALO BULLS.

Bulls of this type are larger and "smoother" in build than the average full-blood bison bull; they are wider between the front legs and thicker between the quarters, and stand straighter on their hocks. Frequently they are not fertile, or only imperfectly so. In the hands of the butcher they will cut about 70% net of their gross weight, according to Col. Goodnight. The bull here shown was bred by the Mossom Boyd Co., and is by a pure buffalo out of a hybrid (first generation) cow. (Figure 5.)

DANGERS OF FIRST CROSS.

The most remarkable and the most disastrous feature of the first stage was the abnormal secretion of the amniotic fluid which was excited without exception in every cow and which proved fatal in a large percentage of cases. This feature was so calamitous that others should be warned against attempting this severe cross. We began without any suspicion of the evil and had we not gone to work unwittingly on a large scale, the experiment would probably have failed in the first stage.

Fortunately no trouble of this sort was experienced in the second stage and none to date in the third stage; but we had an interesting reminder of it through crossing our five-eighths-buffalo bull on three domestic cows in 1911. One of these cows swelled to proportions which indicated the former trouble, but it did not prove serious and it is possible that the diagnosis was wrong. The other two cows did not attract any notice in this respect.

FERTILITY OF BULLS.

On the subject of fertility or infertility of bulls I have not much data. The percentage of male births in the first stage was so small as to be remarkable and excite curiosity as to a possible cause. Out of forty-five hybrids only six were male, i.e., about 15%, and we may add to this experience that of Charles Goodnight, who wrote me in 1900 that he had raised many female hybrids but had never one male born alive. Of our six, three died at birth, one within 24 hours, and of the two raised one proved barren after having been thoroughly tested. The other was killed at an early age without having been tested.

In the second stage the proportion of male births rose to 40%. Out of 26 one-quarter-buffaloes, ten were males.

Of these only four were tested and one of the four was found normally fertile. The other three were barren up to 36 months, 32 months, and 25 months of age, respectively, but should have had a longer trial.

Of five three-quarter-buffaloes one only was a bull and this bull, born in March, 1905, was killed by an accident in September, 1908, i.e., at the age of three and one half years. Although given a fair opportunity during the seasons 1907 and 1908 he did not appear to be fertile, but should have been further tested.

Of one-eighth-buffaloes, the only one tried was thoroughly fertile.

One five-eighths-buffalo bull was bred in 1905 from the cross of a bison bull upon a one-quarter-buffalo cow. This cross was interesting as being our nearest approach to a repetition of the severe cross of the first stage. It proved that the 25% of bison blood in the cow was sufficient to make the cross perfectly successful; for gestation was normal and the bull is more or less fertile. We had one calf by him in 1911 and four in 1912. His pedigree is interesting and I give it here:

QUINTO PORTO

Brindle with white face
Born May 23, 1905

dam Quarto Prettymaid
g. d. Hybrid Prettymaid
3 d. Prettymaid 19th 55397 Am. Hereford Record

Sire Banff 2nd (a pure Bison)
by Tecumseh 63518 Am. Hereford Record
by Bonie (a pure Bison)
by Cherub 4th 32333 Am. Hereford Record

It will be seen that he is a mixture of pure buffalo and pure Hereford blood. He is a whole-colored brindle except for the white front to his face; and like the hybrids he is larger and "smoother" in build than an average bison bull; is wider between the front legs and thicker through the quarters; stands straighter on his hocks, and has an immense back.

We have a yearling bull by "Quinto Porto" out of a registered Hereford cow.

FERTILITY OF COWS.

Of 24 hybrid cows, 15 were barren and of the remaining nine only three were regular breeders. These three, all born in 1895, are now 18 years old, and were regular breeders up to last year; having to their credit nine, ten and twelve calves respectively.

Of 12 one-quarter-buffalo cows got by using domestic bulls on the fertile hybrids only one was barren, four were irregular breeders, and seven normally prolific.

Of four three-quarter-buffalo cows got by using a bison bull on the hybrid females only one proved barren, and this one was by a sire which was himself a very poor breeder, for he got no other produce although he had the run of the herd for four months.

The tables on page 197 are given for comparison with the table for the hybrids given in my former report.

The average age at which the *hybrids* produced their first calf was four years.

Of five one-eighth-buffalo cows, one was beefed at four and one-half years of age without having bred, and four were found normally fertile.

As to the fertility or infertility of cattalo bulls we have no information at all. Of 73 cattalo births to date, 30 were bulls. Six of these are being kept for testing but no testing has been done yet. The first females have just come to the breeding age. Of two born in 1907, one gave a calf in 1911 and the other was sent to the butcher December, 1912, as being probably barren. Of four born in 1908, only one has given a calf as yet. We have besides a calf from a three-year-old, and one from a two-year-old. This is sufficient to indicate that even if not so much so as the grade buffaloes (hybrids excepted) which were just about normal, the cattalo females will be fairly fertile; and there is little doubt as to finding a sufficiency of fertile bulls. Complete fertility, if it is not found at once, will doubtless come through selection, as will early maturity.

It does not seem unreasonable, therefore, to suggest that the fur of the bison and his great back may be carried by

A TRUE CATTALO CALF AND ITS MOTHER.

The sire was a grade buffalo (exact pedigree unknown), while the mother is three-quarters buffalo from a first-generation hybrid cow by a pure buffalo bull. This heifer having mixed blood in both lines of ancestry, is therefore a true "cattalo." The photograph, like all those preceding, was made on the ranch of the Mossom Boyd Co., in Ontario. (Figure 6.)

means of selection without any diminution through succeeding generations of diminishing bison blood until the coat and hump have been practically taken from the bison and placed upon the back of the domestic ox.

BREEDING RECORD OF ONE-QUARTER BUFFALO COWS, DECEMBER 31, 1912.

Cow Born	1902	1903	1904	1905	1906	1907	1908	1909	1910	1911	1912	Age of dam when she produced her first calf. Yrs.	Months
July 1899	B	....	C	beefed	Dec.	1905						2	9
Aug. 1899	....	beefed	Dec.	1903									
Apr. 1900	C	B	B	....	C	C	....	C	C	C	....	2	3
June 1900	.....		B	....	....	beefed	Dec.	1907				3	6
Mar. 1901	....	B	....	B	....	C	C	B	B	B	C	2	2
Apr. 1901	....	C	....	C	B	B	A	B	....	C	....	2	1
May 1901	....	C	.....	....	beefed	Jan.	1906					2	
Mar. 1902	.....		C	....	C	B	C	beefed	Dec.	1911		2	2
Apr. 1902	.....		.....	....	C	....	B	B	B	C	....	4	4
May 1904	.....		.....	.....	....	....	A	beefed	Dec.	1908			
June 1906	.....		.....	.....	....	....	C	....	B	C	B	2	2
June 1906	....	.....	.....	.....	....	....	B	beefed	Dec.	1911		2	
												2	6½

B—Bull calf. C—Cow calf. A—Abortion.

The double line divides the grade buffalo calves from cattalo calves.

BREEDING RECORD OF THREE-QUARTER-BUFFALO COWS, DECEMBER 31, 1912.

Cow Born	1909	1910	1911	1912		Age of dam when she produced her first calf. Yrs.	Months
Apr. 1903	.....	.....	.....	.....	Barren	.....	.....
Mar. 1905	.....	A	C	.....		5	.....
Mar. 1905	.....	.....	.....	B		7	.....
May 1905	.....	C	.....	.....		4	11
						5	8

MY EXPERIENCE WITH BISON HYBRIDS.

CHARLES GOODNIGHT, *Goodnight, Texas.*[3]

I CAN summarize my experience in crossing Texas buffaloes with native cattle, as follows:

I take a male buffalo calf, put him with a native cow and let him suck her until weaning time. I let him run with common cattle until large enough to serve. He will then cross with any kind of domestic cattle. In making the first cross, no male calves have ever been born; cows conceiving them either suffer abortion or die, hence I only get heifer calves and a small per cent. of them. There is no trouble

[3]See also "Breeding Cattelo" by C. J. Jones, in American Breeders' Association Annual Report, vol. III, p. 161, 1907.

CATTALOES ON THE RANGE AT THE GOODNIGHT RANCH IN TEXAS.

Col. Charles Goodnight began to domesticate bison in 1880, and produced his first hybrids in 1885. C. J. ("Buffalo") Jones began in 1885 by purchasing hybrid calves in Manitoba; both men are still engaged in the work, which hitherto has proved so costly and unsatisfactory as to lead William T. Hornaday to say in 1904, "It is now quite time that all such experiments should cease. It has been proven conclusively that it is impossible to introduce and maintain a tangible strain of buffalo blood into the mass of western range cattle." (Am. Nat. Hist., p. 103.) Since then, further progress has been made and there now seems to be more hope than ever of a successful outcome of this experiment in hybridization, which has been in progress for at least 125 years without having produced results on a commercial scale as yet. (Figure 7.)

whatever in giving birth. The cause of abortion and death is unknown to me. The heifer calves obtained breed readily to either the buffalo or the cattle. When bred to the buffalo, the males, which are three-quarters, are not fertile. The females are perfectly fertile and will breed to either race. I breed them back to the polled Angus stock from which they come, and get males which are fertile and which are half-breeds.

The type or race of hybrid cattle is now virtually established in a small way and I herewith give you a few of the points of advantage which the "cattelow"[4] have over common cattle.

First, they are immune from all diseases as far as I have tested them. I know they are immune from Texas Black-Leg and Texas Fever. I have shipped three bulls, one-eighth buffalo, to our coast, the worst tick-country we have. One died and two are living, so I have reason to believe that if they were a little higher in the buffalo blood, they would be entirely immune.

Second, the "cattelow" are much greater in weight, eat much less and hold their flesh better under more adverse conditions. They will easily cut about 70% net of their gross weight. They have a better meat, clear of fibre, and it never gets tough like beef.

They have long and deep backs, enabling them to cut at least 150 pounds more meat than other cattle. More of them can be grazed on a given area. They do not run from Heel Flies nor drift in storms, but like the buffalo, face the blizzards. They rise on their fore feet instead of their hind feet. This enables them to rise when in a weakened condition. They never lie down with their backs down hill, so they are able to rise quickly and easily. This habit is reversed in cattle.

When a herd on range gets weak and poor towards spring, their lying down with their backs towards slopes and on sides of hills causes a loss of from 1% to 6%. Every weak cow which so lies down can never get up, unless she is found by the herder.

The buffaloes have 14 ribs, giving them a longer and deeper loin. As we get them higher and deeper in the buffalo we get the extra rib on the "cattelow." They can exist on less food or salt than cattle, as before stated.

They could do without water much longer than cattle, without inconvenience. They are docile, easily broken and never fight. They put on flesh faster than any cattle and will live and appear to do well where cattle will perish. They have many other points in their favor too tedious to mention.

Genetics Must Come First

In studying the inheritance of the more simple physical characters in man, it is evident that we are as yet only feeling our way toward the solution of certain larger and more complicated problems which are of vital importance to the human race. The future of eugenics depends very largely on the solution of these problems. I do not wish for one moment to suggest that the art of eugenics has been born before its time, but I do feel that before we can venture to apply the scientific principles of genetics to human life, we must first make our foundations sure. For this reason, I am convinced that a good deal of spade work in human genetics will have to be done before any considerable amount of practical good can be accomplished in eugenics. Eugenics is simply applied genetics, and *sound eugenics can only be founded upon sound genetics*.—C. C. Hurst: Mendelian Heredity in Man (1912).

[4]The word is so written in Col. Goodnight's manuscript. The spelling "cattalo" has now been generally accepted, after some years of confusion, and is adopted by practically every authority. It is the preferred spelling of this Association.—The Editor.

AN APPLE CHIMERA

Two Varieties Combined in One as Result of Grafting—Fruit Does Not Seem to Fall Wholly in Either of General Divisions of Chimeras—An Explanation of the Phenomenon.

W. E. Castle,
Bussey Institution, Harvard University, Forest Hills, Mass.

ABOUT five years ago, S. A. Starratt casually mentioned to me having seen in Nova Scotia apples which individually consisted of two very distinct varieties of apple separated by a sharp line of demarcation. The statement interested me much as I recalled Darwin's description of such fruits and his uncertainty as to whether they were to be regarded as graft-hybrids or eversporting seedling hybrids. Upon my expressing this interest Mr. Starratt kindly volunteered to secure me fuller information about the peculiar apple tree. As a result I received in the spring of 1909 from W. W. Clarke of Bear River, Nova Scotia, a letter, from which I quote below, together with a package of scions cut from the freak apple tree (or rather trees, for Mr. Clarke's letter indicated that there were several of them and of two different sorts). Some of the scions I was able to graft successfully upon trees in my orchard, but they have not yet borne fruit. In the letter already mentioned, Mr Clarke says:

"About a month ago S. A. Starratt, Esq., wrote me concerning some apples of a peculiar growth I showed him a few years ago. The peculiarity was. that of the apples grown upon the same stem, portions, perhaps, would indicate different varieties. For instance, one end of the apple would have all the characteristics of the Boston Stripe; the other end would be Russet; and where the joining took place there generally was a small ridge, seemingly, because one variety was larger than the other, and that Nature was doing her best to make a good job of a misfit. I did not grow the apples myself, but do know different trees in the orchard, say ten or a dozen, produced the kind of apple referred to. The man who grows these apples says the parent tree was grown from seed and grafted with Boston Stripe, except one tree which was grafted with Greenings, but grown from seed. Mr. Starratt wished me to send you some scions, which I have done today by one of our schooners that is due to arrive in Boston about the 24th to 26th inst. I have instructed the Captain to express direct to you immediately he arrives.

"The scions are in one package but separately tied; the ones marked Greenings are off the limb that produced the hybrid apples last autumn, the Boston Stripe are off the trees that produced the freak apples, but cannot say if the same limbs. No one tree produces any great quantity of the apples referred to, perhaps two or three dozen on the whole tree, the balance will be Boston Stripe or Greening pure and simple. I will undertake to send you some of these apples this autumn when grown."

Apparently Mr. Clarke was unable to secure any of the peculiar apples in the fall of 1909, but two years later, in November, 1911, I was pleased to receive a package containing half a dozen of them, three of which are shown in the photograph kindly made for me by E. W. Nelson. With the apples came a letter as follows:

PARENTAGE OF THE FREAKS.

"I am mailing you today a sample of the Freak Apples. I understand the parent stock was what is known as Boston Stripe into which was grafted Golden Russet. You have the result.

A PLANT-CHIMERA: TWO VARIETIES OF APPLE IN ONE.

Golden Russet and Boston Stripe combined in the same fruit, as the result of a graft. Trees producing these apples bear only a few fruits of this composition; the rest of the crop belongs entirely to one or other of the two varieties concerned. The explanation of these chimeras is that the original buds of the scion failed to grow, after the graft was made, but an adventitious bud arose exactly at the juncture of stock and scion, and included cells derived from both. These cells grow side by side but remain quite distinct in the same stem, each kind of cell reproducing its own sort. (Figure 8.)

I notice these all show the G. Russet at the stem end. I have seen the blossom end Russet. I may say the whole tree does not produce all like the sample, since the Russet predominates. The fruit is not as large as it should be, for the trees lack the necessary culture."

The account given in this letter does not agree altogether with that given in Mr. Clarke's earlier letter, as to the parent varieties. The earlier account stated that Boston Stripe grafted on a seedling gave rise to the parent tree; in this account Boston Stripe is the stock and Golden Russet the scion. It is impossible at present to decide between the two accounts. The photograph shows very well the sharply marked division of the apples into regions very different in appearance. The stem end of the apple was in each case of "russet" and without stripes; the blossom end was smooth skinned, of a light red color striped with yellow or green (uncolored). When one of the apples was cut open the flesh was found to be different in texture and flavor in the two regions. Underneath the russet skin the flesh was coarser grained. One end of the apple was distinctly sour, the other insipid, almost sweet, but I neglected to note *which* region was sour, the russet or the striped portion, but I think the former. The line of demarcation within the flesh was sharp, as on the surface.

Since the time when these "freak" apples first came to my attention the nature and origin of such plant creations has been investigated with brilliant success by Winkler and Baur in Germany. As a result we now call such things *plant-chimeras*, and we know that they arise in consequence of grafting, in cases wherein the original buds of the scion have failed to grow, but an adventitious bud has arisen exactly at the junction of stock and scion and including cells derived from both. Each of these two kinds of cells reproduces its own sort, though they remain side by side but quite distinct in the same stem.

Winkler has produced several different sorts of chimeras by grafting the tomato (Solanum lycopersicum) and the nightshade (S. nigrum) one upon the other and then decapitating the graft. In some of these the stem is divided right and left between tomato and nightshade tissue, the stem on one side bearing tomato leaves, on the other nightshade leaves. Such plants are known as *sectorial* chimeras. In other cases, tomato cells form the one or two outer layers of the entire plant, while underneath occur only nightshade cells; or these relations may be exactly reversed. Histological study fully confirms this view. Such chimeras are known as *periclinal*.

In the apples figured the chimera would seem to be sectorial rather than periclinal, if the line of demarcation ran lengthwise of the apple rather than across it. But as it is, they resemble more a periclinal chimera which has everted its deeper lying tissue at the blossom end of the fruit. Does the fruit of the apple develop in that way? Will such fruits give us a clue to the answer? They certainly merit further study.

Galton's Law of Regression

The law of regression tells heavily against the full hereditary transmission of any gift. Only a few out of many children would be likely to differ from mediocrity as widely as their mid-parent, and still fewer would differ as widely as the more exceptional of the two parents. The more bountifully the parent is gifted by nature, the more rare will be his good fortune if he begets a son as richly endowed as himself, and still more so if he has a son who is endowed yet more largely. But the law is even handed; it levels an equal succession tax on the transmission of badness as of goodness. If it discourages the extravagant hopes of a gifted parent that his children will inherit all his powers, it no less discountenances extravagant fears that they will inherit all his weakness and disease.—Francis Galton: Natural Inheritance (1889).

SEXUAL INEQUALITY IN HEMP

Male Plants Die While Females Grow Vigorously Under Same Conditions—Possible Sex-Limited Environmental Character Suggested—Advantage to Females of Death of Males—Reversal of Usual Effects of Selection.

O. F. Cook,

Bureau of Plant Industry, U. S. Department of Agriculture, Washington, D. C.

A STRIKING example of sexual inequality was noticed in August, 1912, in a plot of hemp that was being grown on the Virginia Truck Experiment Station, near Norfolk. The nature of the inequality will be apparent by reference to figures 9 and 10. All of the male plants were very slender and spindling, and the foliage was of a pale yellowish green color, in strong contrast with the deep blackish green of the female plants. In addition to their more robust form, the female plants were often a foot taller than adjacent males, the largest of the female plants attaining about three feet. Many of the male plants had already died at the time the photographs were taken, August 13, and the others were evidently to follow shortly. But all of the female plants were still fresh and vigorous, and were growing and flowering under conditions that were bringing the male plants to an early death.

To describe the female plants as stronger and more resistant than the male plants seems a rather inadequate statement of so great an inequality of behavior. If two species were tested and were found to react so differently we would say that one of them was adapted to the conditions, and that some essential character was lacking in the other. With two sexes of the same plant behaving so differently, the idea of a sex-limited environmental character is suggested. Certainly there is no general reason or analogy for believing that sex alone would explain so great a difference of behavior under the same conditions.

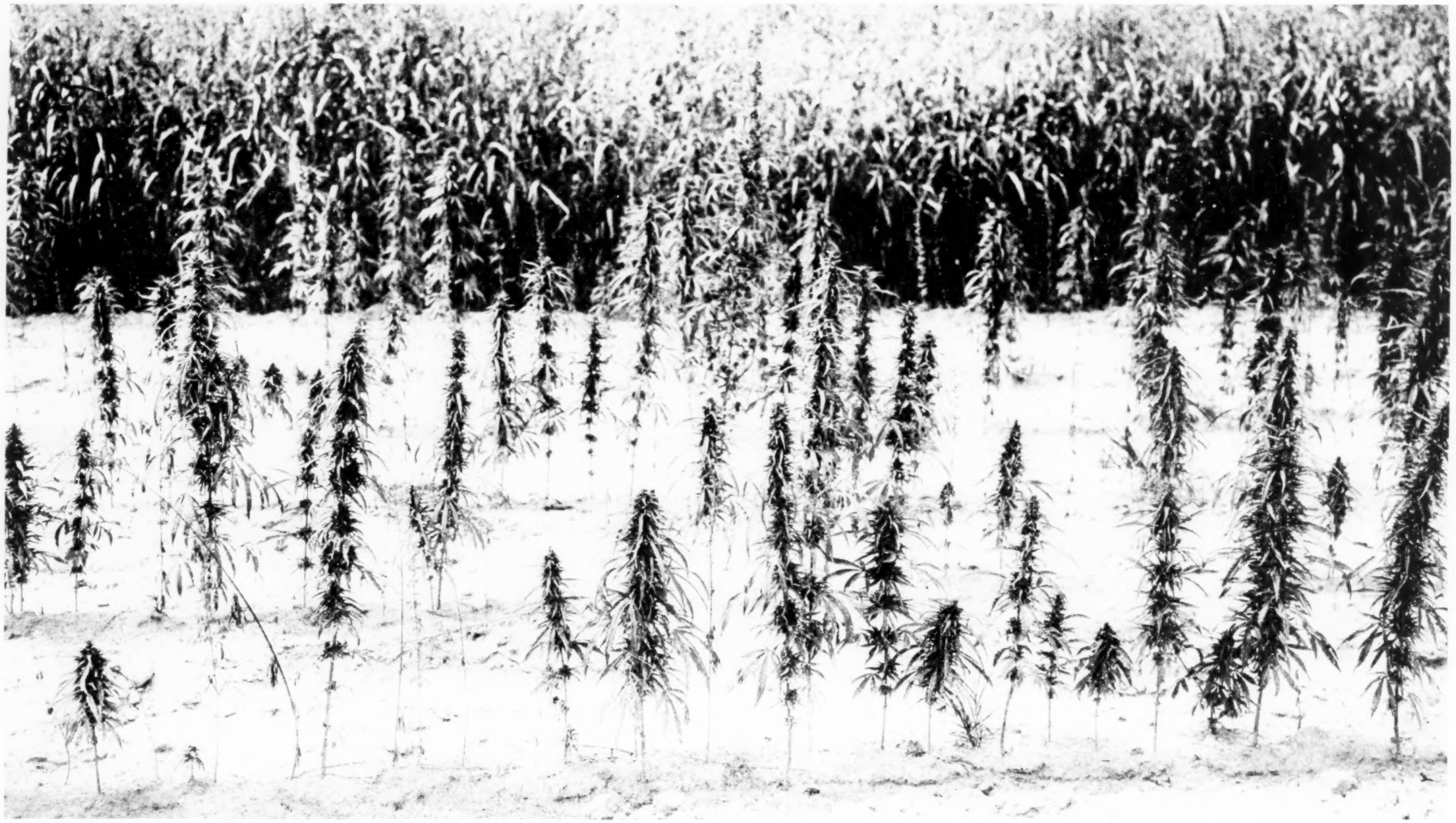
In the perennial plants that have the sexes represented by separate individuals, as among the dates, figs, carobs, poplars, rubber trees, etc., the male individuals appear to have greater vigor and longevity than the females. No doubt it would be an advantage in an annual plant like the hemp to have the males die out after they have shed their pollen. The early death of the males means less competition with the adjacent female plants while these are engaged in maturing seed. The more unfavorable the conditions the more desirable that the males die early in order to increase the chances that the seed be matured. But the advantage of having the males die earlier would not of itself cause them to die. Some other specialization or intensification of the sexual differences must have arisen. On account of the factor of competition between the sexes the usual effects of selection would be reversed. Under extreme conditions short-lived male plants would be likely to leave larger progenies than long-lived males, for the female plants that stood next to short-lived males would be able to ripen more seed. The natural result of such variation and selection would be the development of a plant with short-lived, ephemeral males corresponding to drone bees and similar specializations of the sexes among insects and other invertebrate animals.

WHY THE PLANTS DIE.

It would be interesting to determine the extent to which the presence or the competition of the female plants may be responsible for the early death of the male plants. This could be learned by

FEMALE AND MALE HEMP PLANTS.

The three females (at the left) are growing vigorously, while the three males (at the right), under identical conditions, are dying or already dead. The difference in behavior is so extreme that Mr. Cook suggests a possible sex-limited environmental character to account for it, since it seems improbable that sex alone would explain so great a difference of behavior under the same conditions. (Figure 9.)



SEXUAL INEQUALITY IN A FIELD OF HEMP.

Plants of a Chinese form of hemp, grown at Norfolk, Virginia, and photographed in August, 1912. All of the leafy plants that are readily seen are females, the very slender, thread-like stems representing the males. It seems to be an advantage to the species (this being an annual) to have the males die as soon as they have yielded their pollen, for their competition is thus removed and the females given a better chance to produce seed. Mr. Cook suggests that a reversal of the usual effects of selection would develop a race of plants with short-lived, ephemeral males corresponding to drone bees. (Figure 10.)

raising some of the male plants in plots by themselves. If male plants were found to live longer alone than with the female plants, it might be inferred that they are starved or strangled by the female plants, just as the dwarf male spiders are often killed and eaten by their huge and hungry mates. In both cases the death of the males contributes incidentally to the welfare of the progeny, whereas their continued existence would not.

OTHER VARIETIES LESS UNEQUAL

In the ordinary varieties of hemp grown for fibre or drug purposes there is no such striking inequality of the sexes. Though the male plants die somewhat in advance of the females, they attain nearly the same stature and live through most of the season. The variety that shows this extreme inequality of the sexes is recognized by L. H. Dewey as the so-called Russian or Manchurian hemp, which is extensively grown in Europe and Asia, not for the fibre, but for seed. The oil extracted from the seed is used for food and also has rapid-drying qualities that render it of interest to manufacturers of paint and varnish. The plant has not been grown on a commercial scale in the United States, but attention is now being given to this possibility. The seed crop is much larger than with the varieties grown for fibre, for which the earlier mortality of the males may be partly responsible. Indeed, if the sexes had an equal development it would be hard to believe that a dioecious plant could compete with a bisexual species in seed production. This may be a reason why there are relatively few dioecious plants among annuals.

With perennial plants there could be no corresponding advantage in having the males short-lived. The existence of perennials is not so acutely dependent upon the production of seed every year as upon the ability to gain and hold ground against the competition of other species. In this struggle, large, long-lived male plants doubtless have advantages that more than counterbalance the factor of competition between the sexes. But even with perennials the general tendency of selection is probably in favor of the bisexual types and against the separation of the sexes in different individuals.

SHORTENING OF INTERNODES.

Another peculiarity of the oil-seed hemp is that the lateral fruit-bearing branches have extremely short internodes, analogous to those of the so-called cluster varieties of cotton. The shortening of the fruiting branches may also be connected with the earliness and productiveness of the crop, from which high yields of seed are obtained. This form of hemp, according to Mr. Dewey, is probably the same as that described by Pallas as *Cannabis erratica*, though it is not usually considered as a species distinct from the ordinary *Cannabis sativa*. It has probably been cultivated in China since a very remote period. References to the sexual diversification of the hemp plant in Chinese literature are dated as far back as 2700 B. C.

NOTE.—The Editor has called my attention to a paper that is to be published in the same issue of the Journal, in which a theory of sex-determination appears to have been based largely on experiments with hemp. The inequality of the sexes naturally suggests the question whether the results ascribed to sex determination might not be due to other causes, such as unequal susceptibility of the seedlings to adverse environmental conditions.

Lectures Are In Demand

Announcement in a recent issue of the JOURNAL OF HEREDITY that a lecturer was now available for talks on eugenics, has led to a gratifying demand for the services of A. Edward Hamilton, of the Eugenics Record Office, Cold Spring Harbor, Long Island, New York. He can be secured by clubs, colleges and other organizations which wish to give serious attention to eugenics; his services are made available through the liberality of Mrs. Huntington Wilson, of this association.

THE DETERMINATION OF SEX

Botanist of Lemburg Professes to Have Found That Age of Pollen at the Time of Fertilization is Determining Factor—Claims High Percentages in Experiments—Has Tested Discovery in Animals and Man, He Says.

P. J. WESTER,
Horticulturist in Charge of Lamao Experiment Station, Philippine Bureau of Agriculture.

THE determination of sex is a riddle that for ages has puzzled the minds of men and numerous are the theories that have been advanced in an attempt to explain which factors determine the sex in the progeny—theories advanced only to be abandoned as untenable in the light of further experimentation. It has long seemed and by many been asserted that sex determination was a mystery held in trust by Nature that was forever to remain unravelled[1]. It now appears that while it has continued to occupy the minds of many scientists, the problem may in reality have been solved for some years. This remarkable discovery, which unquestionably will mark a new epoch in animal breeding no less than in the eugenics of the human race, if the apparently simple law involved is upheld by thorough tests and becomes generally diffused, was made in 1878 by Prof. Theophilus Ciesielski in Lemburg as a result of continued experimentation begun in 1871.

The earliest investigation by Ciesielski was conducted on Indian hemp (*Cannabis sativa*) and garden spinach (*Spinacia oleracea.*) It is not here necessary to go into detail regarding the earlier years of experimentation which yielded negative results to the investigator. In a paper published first in Latin and later translated into English, in which form it appeared in International Clinics, Vol. 3, 22nd series, 1912, prefaced by a review by Dr. S. W. Carruthers, of Norwood, England, Ciesielski thus relates the circumstances under which the discovery of the law of sex determination was made:

"As soon, then (1876), as the sex of the plants *Cannabis sativa* . . . could be determined I took out all male plants from the . . . garden, leaving only female ones, . . . Thereafter, as soon as the female plants were mature I fertilized them with pollen from the male plants . . . simply by shaking the male plants over them. The female plants in the upper garden I fertilized daily at sunrise, but those in the lower garden just before sunset.

HIGH PERCENTAGE SECURED.

"The seed collected from those divisions I sowed again in 1877, separately in the upper and lower portions of the garden, but in new plots, to avoid any possible fallacy from seeds of the former experiment casually and spontaneously shed in the old plots. The result was that from seed derived from plants fertilized at sunrise I obtained 85.5% of male plants, while the seeds

[1]For the benefit of readers who are not in touch with recent biological researches, it should be said that many of the investigators who have given the most study to the question believe the determination of sex depends on the inheritance of a Mendelian factor differentiating the sexes. Microscopical studies of the cell indicate that the presence of an accessory or "x" chromosome in the sperm or egg (usually the latter) is the deciding factor, in some species. If sex does Mendelize, the proportion of males and females is thus subject to the laws of chance—in other words, an equal number of each will be produced, if the quantities involved are large enough to furnish accurate statistical results, and if differential mortality does not take place.—The Editor.

gathered from plants fertilized at sunset produced 92% of female plants.

"In view of the success of these experiments in fertilization I took up, in 1877, six female plants with their roots, before they had fully produced their flowers, and transplanted them into pots; as soon as the plants were well rooted I transferred the pots to the windows of two rooms looking south, three pots in each room. Thereafter, with a brush, I fertilized the three plants in one room with pollen which I collected from anthers just dehiscing and not yet fully open (this pollen I shall refer to as fresh), and the three pots in the other room I also fertilized by means of a brush, but with pollen taken in the morning and kept in paper till evening. The three plants fertilized with 'fresh' pollen produced 120 seeds; the three fertilized with 'stale' pollen produced 96 seeds.

"In 1878 I sowed in my garden with great care the seeds of the two lots separately at distances of 20 centimeters. The 120 seeds derived from fertilization with fresh pollen produced 112 plants, of which only six were female, all the rest being male. But the other 96 seeds, sprung from fertilization by stale pollen, produced 89 plants, every one of which was female. I have several times since repeated the experiment, always with a similar result."

Having reached this result in plants, Ciesielski extended his investigations to animals, and tested his theory in numerous experiments with rabbits, dogs, horses, and cattle, an overwhelmingly large number of which, he states, served to verify his previous results. Finally, Ciesielski declares that he has repeatedly recorded the application of the same law in man also.

To sum up: According to Ciesielski the sex of the progeny is governed by the conditions of fecundation and the law discovered by him applies to all living beings, whether plants, animals or man. In dioecious plants fresh pollen produces male seeds, while the effect of stale pollen is the production of female seeds. Similarly the sex in animals is determined by the age of the spermatozoa at the time they unite with the ova.

In plants this discovery will be of more or less academic interest, but in its relation to animals and man the results are sure to be important and far-reaching—*if it endures the light of the continued experimentation* to which this new "law" is already being subjected.

Nature and Time of Segregation

That segregation of unit characters may occur in somatic divisions is the contention of William Bateson. Discussing the subject before the seventeenth International Congress of Medicine, he said:

"First, as to the nature of segregation. This, I think, we must regard as a process comparable with the mechanical separation of substances which will not mix, or mix imperfectly; whereas some factors are continually transmitted in their entirety, others are liable to be broken up by what I regard as a process of quantitative fraction occurring in the mechanical dissociation of the elements at certain critical cell divisions. As to which are the critical cell divisions, we have no clear indication. I cannot agree with those of my colleagues who think segregation must occur exclusively in the maturation processes. The case of the double stock, in which the whole male side of the plant differs genetically from the female side, as proved by Miss Saunders, shows almost conclusively that segregation may occur in somatic divisions. It is also difficult otherwise to interpret the fact that in certain cases the parental combination influences the distribution of factors among the gametes so that the distribution among the grandchildren is different according to the way in which the characters were combined in their grandparents."

WEAKNESS OF ELDEST SONS

Statistics of Physical Examinations at Amherst College Seem to Bear Out Theory That Younger Sons are Physically Superior to the First-Born—Age of Parents Does Not Appear to Exercise Much Influence.

JOHN H. CHASE, *Youngstown, Ohio.*

FEW factors in constructive eugenics have been more emphasized during recent years than the desirability of large families among superior people; and this factor received particular attention when English biometrists published studies purporting to show, as one reason for the advantage of large families to society, that the elder children were inferior to those who followed; and, consequently, that the general average of the family, if small, was considerably below what it would have been if larger.

Karl Pearson, of London, the most noted proponent of this theory, was convinced that first and second born children are on the average inferior to their later born brothers and sisters, not only physically, but mentally and morally as well. In his pamphlet on "The Problem of Practical Eugenics," Pearson said:

"If our observations are correct, and I believe them to be so, then the mental and physical condition of the first and second born members of the family is differentiated from that of the later members. They are of a more nervous and less stable constitution. We find that the neurotic, the insane, the tuberculous and the albinotic, are more frequent among the elder born. Dr. Goring's results from criminality show the same law. . . . In the tuberculous, the insane and criminal stocks, the first few members are weighted. But the result of this new law is remarkable. It means that if you reduce the size of the family, you will tend to decrease the relative proportion of the mentally and physically sound in the community."

A proposition of such direct application and popular interest naturally aroused widespread attention. T. B. Macaulay, a Montreal member of the American Genetic Association, critically analyzed the statistics,[1] and declared that the conclusions drawn from them were a mere statistical fallacy. Others followed him, until the method of compiling the statistics on which the theory rests was to a large extent discredited.

This method was to take the records of some institution—a sanatorium or penitentiary, for example—as a basis. The inmates were questioned as to the number of children in the families to which they belonged, and their own order in the families. The number of first-born inmates was compared with the total number of first-born children in the families; and so on with the second, third and later born. The percentages thus found have been higher in the case of early born inmates than of later born. It has been pointed out, however, that this result has no significance, and must necessarily follow, because the younger members of families are, quite naturally, not as numerous as the older ones in groups of adults, whether such groups be undesirable, as the tuberculous, the insane or the criminal, or unobjectionable, as doctors, lawyers, clergymen, heavyweights, married men or fathers. By statistics compiled in this way, it is argued, it would be possible to "prove" that elder born children are superior to the younger born, quite as easily as that they are

[1] In the American Breeders' Magazine, Vol. II, no. 3, p. 165, July, 1911. He also called attention to the matter at the Eugenics Congress in London, 1912.

inferior. The theory thus falls to the ground, in the opinion of many, from lack of proof.

With the desire to test the theory by a method which would obviate the objections raised against that of Pearson and his co-workers, I investigated the records of the physical examinations of students who have entered Amherst College for a number of years past. At this institution Dr. Edward Hitchcock was the first man in charge of a collegiate physical department in America to establish anthropometric measurements and strength tests. He worked out a system of examination which was so thorough that there has been found no reason for changing the tests in more than 40 years. Each incoming freshman class has been measured and tested in the same way during all that time.

Thus a disinterested and reliable means of making an investigation as to the comparative strength of brothers was furnished; and as all those included in the data are adults, to the extent at least of being old enough to enter college, the fallacy pointed out by Macaulay in Pearson's studies is here obviated.

With the cooperation of Amherst authorities, I secured a list of 58 sets of brothers, from the physical examination charts. A correspondence was conducted with these individuals, particularly with a view to learning the ages of their parents, and their own order in the fraternities. Some of these sets of brothers included three individuals; most of them only two. Some of the sets I was obliged to disregard, in various computations, for various reasons.

In grouping the records of strength tests, I found first-born brothers strongest in four cases, second-born brothers strongest in 12 cases, and third-born strongest in 28 cases. As the individuals concerned came to college at about the same age, on the average, it seems clear enough that first-born brothers who came to Amherst were weaker than their younger brothers who followed.

Handling the figures in a different way, it was found that first-born brothers made a total of 11,361 points in the strength tests, second born had 12,510, and third born 13,251. The average strength of each individual in each class was: first born, 494 points; second born, 544 points; third born, 576 points. Of course, with such a small group of men, a few exceptional individuals would make a considerable change in the totals, and we know that there were exceptional individuals. Nevertheless, the results are so consistent as to make me confident that in this case the comparative strength of the brothers is faithfully represented by these figures, which seem to indicate that second-born sons were about one-tenth stronger than first-born sons, and that third-born sons were correspondingly stronger still.

Out of all the records taken, there were only 20 boys who made more than 600 points in the strength test on their entrance to the college, while there were only 14 who were below 450. Suppose, then, we say that those below the 450 mark are weaklings, those about 525 normal, and those above 600 strong. This makes 75 points as the difference between a normal young man and a weakling on one side, and an exceptionally strong man on the other side.

And this is just about the difference in the average strength of the first-born and third-born sons who entered Amherst College.

Taking only the two older of each of these sets of brothers, I added three more similar pairs for which the same data were available, and tested them on height and weight, as well as strength. Without going into details, I give the results:

Younger brothers were heavier in 24 cases; older brothers were heavier in 23 cases.

Younger brothers were taller in 22 cases; older brothers were taller in 25 cases.

Younger brothers were stronger in 30 cases; older brothers were stronger in 17 cases.

The interpretation of these results may afford room for some dispute; but as the disparity in strength is still marked, it seems to me merely to indicate that height and weight have little bearing on the question of strength or

constitutional superiority. I believe this conclusion is not out of harmony with our observations on those about us in daily life.

Of course, statistics on this point are always open to the obvious explanation that children of immature parents are likely to be of inferior physique. That is an entirely different question, which depends on the age of the parents, and not on the order of birth, or the size of the family. However, in order to find whether any light was thrown on my results, I secured information from as many of the brothers as possible—30 pairs or 60 individuals—as to the ages of their parents, and then compared the records of the individual students with the ages of their parents when the individual was born.

Out of the 60 different cases, there were seven men who had records of over 700 points in their strength tests. The strongest man in the group totaled 859 points; I learned that his father was 40 years old and his mother 42, at his birth. The second strongest had 809 points to his credit; the age of his father at the time of his birth was 49 years, and of his mother, 35. Of the other five strong men, in order of rank, the ages of the parents were as follows:

Father 50, mother 30; father 33, mother 31; father 28, mother 28; father 47, mother 33; father 42, mother 28.

The average age of the fathers was 41 and of the mothers 32.

I should be the last one to attempt to lay down any conclusions from such a small number of cases, but I may at least point out that these figures are unusual, if we accept the widely-quoted view that the best period for the production of children is between 20 and 30.

Summing the ages of the parents of the students whose strength I have called "normal," I found that the fathers averaged 38 and the mothers 31 years of age, at the birth of the sons in question. When the weakest students in our list were considered, their parents were found to have been, on the average, 36 years old (father) and 30 (mother) at the time of their birth. In such a small group, the slight decrease in age of parents, correlated to weakness in sons, can hardly be held to have any significance; and that it has no significance in these figures is further indicated by another test I made to check it. I picked out all the parents who had their first child after they were 30 years old, and compared the strength records of these first children with the records of the other first born, who were born before their parents reached the age of 30. The children of older parents averaged 500 points against 494 for the children of younger parents—a difference so slight as to amount to identity. It is obvious, then, that as far as these cases are concerned, the age of the parents has little appreciable effect on the strength of their children; but it must be noted that practically all the parents under consideration were mature, and even considerably beyond the usual age for the beginning of parenthood.

The question whether first-born children are inferior is still far from settled. Assuming for the moment that they are inferior, we still have to seek a cause for such inferiority. One of the most popular explanations does not concern itself with heredity at all, but supposes that the younger children profit by being kept constantly on their mettle, in order to make a favorable showing in comparison with the older ones; the eldest, on the other hand, are not obliged greatly to exert themselves in competition with the younger ones. My investigation of these Amherst students does not, of course, pretend to settle any of the questions involved, but I offer it as a contribution which at least avoids the fallacy charged to Pearson. Summing up, it indicates that the eldest sons are physically weakest, and that this weakness has no relation to their weight or height. If the age of their parents at birth has any influence on the figure, it is slight.

EXTINCTION OF FAMILY NAMES

Algebraical Computation Shows That if World Lasted an Infinite Length of Time, All Inhabitants Would Have Same Surname—Investigation of German Villagers Shows That Process of Extinction, Although Certain, is Slow.

Compiled by the Editor.

THAT every family name must die out sooner or later, was suggested by Alphonse de Candolle many years ago. Sir Francis Galton caught up the suggestion and proved its truth by mathematical computation. A German investigator has lately analyzed the data from 1400 marriages, in order to find out exactly in what way the extinction takes place, and reaches the conclusion that the probable extinction of any family can be foreseen one or two generations before the line actually runs out, and that as a general rule, when the decline has set in, nothing can stop it.

"In the accurate information and sensible statements of Benoiston de Chateauneuf, Galton and other statisticians," said de Candolle in his *Histoire des Sciences et des Savants* (1873), "I have not noticed the very important conclusion which they ought to have made, on the *inevitable* extinction of family names. Obviously all names must finally disappear. A mathematician could calculate how the reduction in the number of names and titles takes place, according to the probability of wholly female or wholly male births, or births of both sexes, and according to the probability of the absence of births with any given couple."

Galton's own reflections on the subject were as follows[1]:

IMPORTANCE OF THE PROBLEM.

"The decay of the families of men who occupied conspicuous positions in past times has been a subject of frequent remark, and has given rise to various conjectures. It is not only the families of men of genius or those of the aristocracy who tend to perish, but it is those of all with whom history deals, in any way, even such men as the burgesses of towns, concerning whom Mr. Doubleday has inquired and written. The instances are very numerous in which surnames that were once common have since become scarce or have wholly disappeared. The tendency is universal, and in explanation of it, the conclusion has been hastily drawn that a rise in physical comfort and intellectual capacity is necessarily accompanied by diminution in 'fertility'—using that phrase in its widest sense and reckoning abstinence from marriage as one cause of sterility. If that conclusion be true, our population is chiefly maintained by the 'proletariat,' and thus a large element of degradation is inseparably connected with those other elements which tend to ameliorate the race. On the other hand, M. Alphonse de Candolle has directed attention to the fact that, by the ordinary law of chances, a large proportion of families are continually dying out, and it evidently follows that, until we know what that proportion is, we cannot estimate whether any observed diminution of surnames among the families whose histories we can trace, is or is not a sign of their diminished 'fertility.' * * * Although I have not hitherto published anything on the matter, I took considerable pains some years ago to obtain numerical results in respect to this very problem."

A MATHEMATICAL SOLUTION.

Galton then quotes the solution of the problem worked out for him by Rev. H. W. Watson, supposing N to repre-

[1]Galton, Francis,' Natural Inheritance, p. 241. London, 1889.

sent the number of adult males in a population, each with a different surname. After complicated computations, which represented five generations, and are far too abstruse to be understood by anyone but a mathematician, he successfully solves the problem algebraically, concluding:

"As, therefore, time proceeds indefinitely, the number of surnames extinguished becomes a number of *the same order of magnitude* as the total number at first starting in N, while the number of surnames represented by one, two, three, etc., representatives is some infinitely smaller but finite number. When the finite numbers are multiplied by the corresponding number of representatives, sometimes infinite in number, and the products added together, the sum will generally exceed the original number N. In point of fact, just as in the cases calculated above to five generations, we had a continual, and indeed at first, a rapid extinction of surnames, combined in one case with a stationary, and in the other case an increasing population, so it is when the number of generations is increased indefinitely. We have a continual extinction of surnames going on, combined with constancy, or increase of population, as the case may be, until at length the number of surnames remaining is absolutely insensible, as compared with the number at starting, but the total number of representatives of those remaining surnames is infinitely greater than the original number."

Mathematics, then, indicates that if the world would only last an infinite length of time, all its inhabitants would have the same surname. It is interesting to see how rapidly the process proceeds in actual experience, even among a people where the birth-rate is high.

A SOLUTION FROM LIFE.

The investigation has been followed out by Dr. Fr. von den Velden[2] from a statistical point of view, on the basis of figures compiled by Riffel[3] concerning 1400 marriages of peasant families in Baden. Considering the nature of the material, it will be admitted that the number of family lines which ran out is much smaller than would have been the case had the data been secured in a large city, or among people of a higher social class. The writer's figures may fairly be considered to represent the extent of family-extinction under the most favorable circumstances for continuation.

He begins by pointing out that we are accustomed to speak of a family as becoming extinct when the male line, and with it the family name, disappears. This is misleading physiologically, because the female line is of equal importance with the male line, to the biologist. He therefore investigates his data in regard to both lines.

Considering the various causes which may contribute to the extinction of a family, Dr. von den Velden says, "The simplest case is when a family remains absolutely childless. The causes of this rather rare occurrence (in the material here used 3% of the marriages are unfruitful) are to be sought in a minority of cases in the fact that one of the partners is of advanced age; more often—probably in two-thirds of the cases—the reason is that one of the partners is weighted by heredity. It is not to be understood that he or she inherits sterility; but sterility ensues as a result of some disease, a tendency to which has been inherited.

MARRIAGES WITH ONE CHILD.

"Equally rare with unfruitfulness is the case when, although children are born, none of them reaches marriage. In the material at hand we find 33 such marriages, or 2.3% of the whole. The average number of births in these marriages is 3.8. Marriages which result in only one child predominate, but on the other hand we find one with

[2]"Aussterbende Familien," von Dr. Fr. von den Velden (Frankfort a M.),in Archiv. f. Rassen u. Ges. Biologie, VI, 3, 340.

[3]Riffel's well-known genealogical-nosological tables were published in Mitt. über die Erblichkeit und Infektiosität der Schwindsucht (Braunsch. 1892) and Schwindsucht und Krebs (Karlsr. 1905).

11 children, all of whom died before the twentieth year, a second with 10 children, eight of whom died young, while the other two were feeble-minded. Of the 87 children included in the class under discussion, 80% died before reaching the twentieth year, 8% are phthisical, and only a single one of the number passed the sixtieth year. This lamentable constitution of children finds its explanation, with the exception of a few cases, in the condition of health of the parents."

"Up to this point we have dealt with the entire extinction of a whole generation, whether due to the production of no offspring at all, or the production of no fruitful offspring. It is more interesting to consider the extinction of the male line alone, since that is commonly identified with the extinction of the family."

"In this connection," says Lorenz[4], "the cases are particularly noteworthy where the deficient production of boys is balanced by an over-production of them in the immediately preceding generations. This happens so frequently that one is inclined to suspect a causal relation." He cites many instances to prove it, and Riffel's tables allow a striking confirmation of his remark. If one groups the fathers of families which consist only of girls (or at least, no boys which reached more than five years of age), the brothers and sisters of these fathers show the following sex proportion:

197 boys to 100 girls (Huttenheim)
146 boys to 100 girls (Stupfaich)
149 boys to 100 girls (Herlsdorf)
160 boys to 100 girls, the average in such cases, taking the total for the three villages.

DISPROPORTION OF BOYS.

Since the average for the whole population is 106 boys born, for every 100 girls, the disproportion of boys in the generation of the father whose family is about to become extinct in the male line, is altogether surprising.

"Very different, on the contrary, is the sex-proportion in the generation of the mothers of families which consist only of girls (not counting boys who died in infancy). Here the girl births are in the majority, and in the two Riffel tables in question we find a proportion of 93 boys to 100 girls, in place of the 106 boys to 100 girls which the general average of births in the empire would require.

"We see, then, that extinction of the male line is preceded (as a rule, and in the average) by a great disproportion of male births in the father's generation, and an overweight of female births in the mother's generation. Using a figure of speech, we may say: The male line blooms itself to death, and the excess of boys is suddenly transformed to an excess of girls, through marriage with a woman in whose family girl births have been in excess. So long as one regards such a statement merely as an aid to memory of the facts, it will not be misleading."

As to the frequency of extinction of male lines in the 1400 marriages now being considered there "are 84 (6%), among the offspring of which only daughters attained marriage. Extinction in the male line therefore seems to have about the same frequency as the complete extinction of a family, since there were found 3% of childless marriages and 2.3% of marriages which produced no fruitful offspring. That only daughters married, in these 84 families, is by no means to say that there were no sons in the same families; but those who existed died before the age of marriage or remained celibate; the latter, however, is an extremely rare occurrence in these villages of Baden. As a matter of fact, a large majority of the families, from which only daughters married, also produced sons—in only 20 out of the 84 were sons completely lacking[5]. In the rest, the sons died young.

USUAL FORM OF EXTINCTION.

"Out of these 84 families which became extinct in the male line but con-

[4] O. Lorenz, Lehrb. der gesamten wissensch. Genealogie, p. 482, Berlin, 1898.

[5] In 44 families only sons married, but in only 14 of these were no daughters born at all.

tinued in the female line, we get 44 in which only one son married, and 18% of these families died out completely in the following generation. In the families where no son married, but one or more daughters married, we find that 33% became totally extinct in the following generation. We can then conclude that the usual form of gradual extinction of a family (as opposed to immediate extinction through childlessness, or through the production of wholly unfruitful children) is usually preceded by the extinction of the male line: first, no more boys are born; later, no more children of either sex—at least none which reach marriage."

"It will now be interesting to compare the children produced during these two different kinds of gradual extinction: first, where the male line is reduced to one individual, and, second, where the male line is extinct, leaving the family to be carried on by daughters alone. The condition of the children is about equally unfavorable in each category. The average number of children is higher, however, when there is still a son who reaches marriage—thus:

With marriage of a single son—6.2 children, 10.3 grandchildren.

With marriage only of daughters—4.8 children, 6.8 grandchildren.

From a survey of all the cases[6] it is further evident that the average condition of health in families whose extent is decreasing, is inferior, and further deteriorates with each generation. In cases where there was only one man in the fraternity who married, there are still 4.6% of the children of such individuals, who passed the sixtieth year without inherited defects; but in the grandchildren the proportion is reduced to 1.6%; and thus the male line runs out."

"Accordingly, where a family has once started to die out, the deterioration of its constituents is progressive—well-marked, on the average, although of course single cases may offer exceptions. That a family threatened with extinction during one or two generations can recover itself, is a matter of every-day knowledge to us: but such a recovery is the exception, not the rule."

Possibility of Quick Returns

The want of systematic study of heredity is due chiefly to misapprehension. It is supposed that such work requires a lifetime. But though for adequate study of the complex phenomena of inheritance, long periods of time must be necessary, yet in our present state of deep ignorance almost of the outlines of the facts, observations carefully planned and faithfully carried out for even a few years may produce results of great value. In fact, by far the most appreciable and definite additions to our knowledge of these matters have been thus obtained.—William Bateson: Mendel's Principles of Heredity (1902).

What Eugenics Promises

There can be no question that among the promises of race-culture is the possibility of breeding such things as talent and the mental energy upon which talent so largely depends. . . . Eugenics can reasonably promise, when its principles are recognized, to multiply the human and diminish the vegetable type in the community. In so doing it will greatly further the production of talent, and therefore of that traditional or acquired progress which men of talent or genius create. Such a result will also further, though indirectly, the production of genius itself. For, as Mr. Galton points out, "men of an order of ability which is now very rare, would become more frequent, because the level out of which they rose would itself have risen."—C. W. Saleeby: Parenthood and Race Culture (1909).

[6]Dr. von den Velden sums up his cases in a table, which is here omitted because of its length.

THE PHILIPPINE NEGRITOS

Three Fundamental Types of Mankind Seem to Enter Into Their Make-up, but the Australoid Predominates—This May be Either the Basic Stock of All Humanity, or a Product of the Fusion of European and Oriental Stocks, the Iberian and the Primitive.

Extracts from two articles by Robert Bennett Bean of Tulane University, New Orleans, La., in the *American Anthropologist*.[1]

THERE are three fundamental types which are found among all the peoples of the Philippines, and the three types I have designated Iberian, Primitive and Australoid. The Iberian is of the classic Greek mold, thin, wiry and trim, and may be differentiated from the other types by the ear form, cephalic index and nasal index. The ear is characterized by the eversion of the concha, the rolling out of the helix, and the spiral shape of the outer rim, as seen from behind; the cephalic index is low the nose long and aquiline. The type is called Iberian because the purest living representatives are to be found in Spain, the Iberian peninsula.

The Primitive is in direct contrast to the Iberian because of the stocky build, usually short, squat and fat; the ear is distinctly bowl-shaped, with depressed concha and rolled-in helix; the head is short and broad, and the nose is short, broad and flat, with depressed root. The type is called Primitive, because it is infantile, because it is called Primitive by Hagen, and because it is found largely among the so-called Primitive peoples of the earth.

The Australoid is neither thin and delicately molded nor squat and fat, but is rather lanky and heavy in body and limb. The ear is large, somewhat bowl-shaped, with a sloping shelf for lobule, and the helix is horizontal in its upper part, making a square bowl instead of a round one as in the Primitive. The head is long and narrow, but the nose is broad, long, high and large in all dimensions. This type is called Australoid because it is believed to be the fundamental type of Australia and of all negroid peoples.

The Australoid is the fundamental Negrito type and it is found purest among the Mariveles Negritos and along the eastern coast of Luzon. It is also found elsewhere throughout the islands in populations that are not Negrito, where it is somewhat modified but has a form that is clearly recognizable. It has been found by me wherever I have observed Filipinos, and is one of the three fundamental types of the islands.

ORIGIN OF THE THREE TYPES.

The origin of the three types is not entirely clear. There can be no doubt that the Iberian is the same as the Mediterranean race of Sergi, derived originally from Europe. The Primitive has a wide range of distribution according to Hagen, being found in South America, Africa, the Pacific islands, and on the mainland of Asia. Wherever found it has associated with it a type resembling the Australoid, therefore the two may have originated together. If they did not, they have at least drifted together over the world and are found together at present over a large extent of the earth's surface. The Australoid type is a large element in the composition of the negro people,

[1]This article is a mosaic of extracts from the concluding portions of two papers by Dr. Bean: "Types of Negritos in the Phillipine Islands" (Am. Anthropologist, Vol. 12, No. 2, 1910) and "Philippine Types" (Ibid., Vol. 12, No. 3, 1910). The Editor is wholly responsible for the selection of these extracts.

FRUIT VENDERS FROM NEGROS ISLAND, P. I.

"It has been asserted repeatedly that the body color of the Negrito is black," says David P. Barrows, "but this is a gross exaggeration. It is a dark brown." "The hair of the Negrito is typically African. It is kinky and grows in the little clusters or 'peppercorn' bunches peculiar to negro races." Dr. Bean says this kinkiness of the hair behaves as a Mendelian dominant in crosses. The Negritos shown in this picture (taken at Camp Jossman) are selling fruits of the Jak (*Artocarpus integrifolia*), the largest fruit known, and closely related to the commoner bread-fruit (*Artocarpus incisa*). (Figure 11.)

judging from observations on many hundreds of American negroes.[2]

The Negritos in the Mariveles mountain appear to be the purest Negritos in the Philippine Islands, judging from the photographs. All other groups of Negritos are more like the surrounding population. The Negritos of Mariveles should therefore represent the fundamental Negrito type, and as they are largely Australoid the fundamental type of the Negritos should be Australoid. Modified Primitive and Modified Iberian Negritos are also found in the Mariveles group. The women are more Primitive than the men, who are more Iberian

[2]"The photographs of the Mariveles Negritos were all placed upon a large table before me, and when I scrutinized them carefully I realized that I was looking into the faces of a familiar people, among whom I was born and with whom I grew up, the American negro. Practically every face recalled youthful associations, and every individual of the Mariveles group has its counterpart among American negroes."—Bean.

and Australoid than the women. The Primitive and Iberian characteristics of the Mariveles Negritos are of such a nature that they should be considered as remnants of the fusion that must have progressed for many centuries or even thousands of years to have produced so homogeneous a blend.

The origin of the Negrito may never be known, but it seems plausible that the Australoid is the true Negrito type on which the Iberian and the Primitive types have been grafted. The Iberian came from Europe by way of India in prehistoric migrations, and their combinations with the Australoid are represented by the Negritos of Mariveles mountain and those of the eastern coast of northern Luzon. The Primitive came from the Orient, and has mingled with the Iberian-Australoid Negrito throughout the Philippines, altering the form and straightening the hair of the purer Negritos that existed before. The more recent European contact with the Negrito has had very little apparent effect, although an occasional pure Iberian may be found with dark skin and kinky hair. The Negrito element has entered to a considerable extent into the present Filipino population, although it may not be recognized with ease because of the absence of kinky hair. The Negritos have been losing their identity by becoming incorporated into the body of the surrounding population. Where once the Negrito was found in all parts of the archipelago, the only remaining centers in which they exist in anything like their original purity are the two already mentioned, Mariveles mountain and the eastern coast of Luzon. In all other parts the kinky hair has practically disappeared and the Negritos have taken on the form of the surrounding population, distributing their own forms throughout the regions roundabout.

CHARACTERS IN HEREDITY.

Any conclusion reached after a study of the photographs presented[3] must be tentative and subject to revision. It does appear, however, that there are many inseparable factors in the composition of the individual, such as the ear-form, nose-form and face-form, and length of the extremities, that constitute a "character-complex" which exists as an entity and in inheritance may act as a unit character.

A "character-complex" is that group of characters, such as the broad head, broad nose, broad face and characteristic ear, that, combined with small stature, constitute the Primitive species, which character-complex usually hangs together in heredity, but may break up when crossed with alien forms to create new character-complexes.

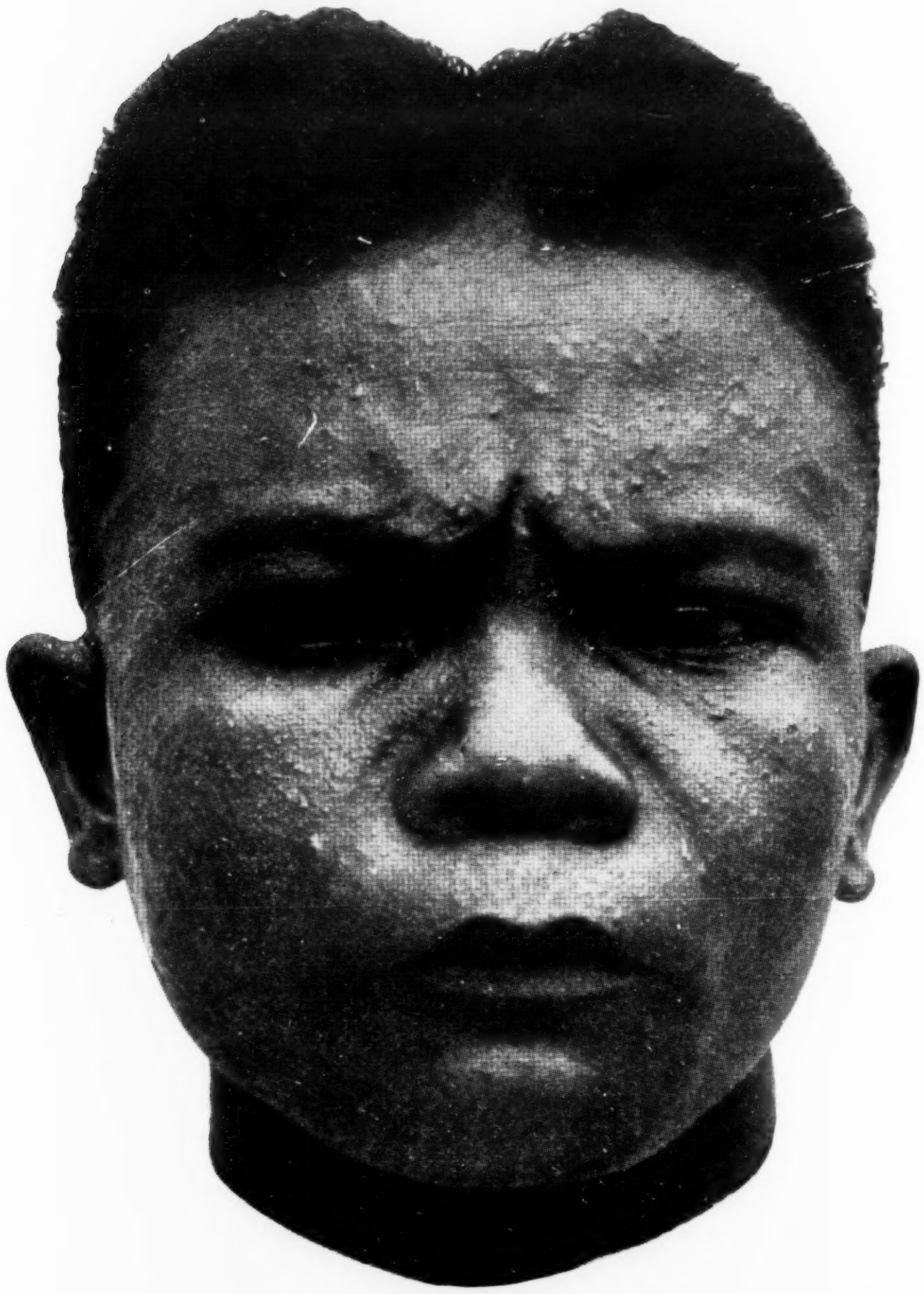
There is blending of one character-complex with another, but this blending probably does not take place at once upon crossing two extremely different character-complexes such as the Iberian and the Primitive, or the Iberian and the Australoid, but results in the reappearance of one or the other character-complex in pure form. . . .

From this follows the second conclusion, which is that the kinky hair is dominant over the straight hair when the cross takes place among the Negritos; therefore a character-complex may be obscured by kinky hair, so that an otherwise pure Iberian may appear to be a Negrito because of the character of the hair.

The following conclusions are tentatively made from observations of Europeans, Filipinos and negroes. The Iberian is the fundamental European type, but modified Primitive and Australoid types may be found among Europeans besides the composite types, such as the Alpine, B.B.B.,[4] etc. The Australoid is the fundamental negroid type, but a great many Iberian and Primitive forms are found among negroes, and the composite types are also present. The Primitive is the fundamental type of the Orient and of the Pacific peoples, but both Iberian and Australoid as well as the composite

[3]This refers to the photographs presented by Dr. Bean in his original articles. He is not in any way responsible for the photographs in these pages.

[4]The B. B. B. is the big-cerebellumed, box-headed, Bavarian of Ranke, so-called by Beddoe. —Bean.



A NEGRITO-TAGALOG CROSS.

Although this man (25 years old, 4 feet, 4¾ inches high) claimed to be half Tagalog, his type is purely that of the Mariveles Negrito, the purest of the Philippine Negritos and most resembling the original Australoid stock. "The only other undisputed members of the Negrito race, besides those found in the Philippines," says David P. Barrows, "are the Andaman islanders and the Semang of the Malay peninsula." "He is a scattered survivor of the pygmy negro race, at one time undoubtedly far more important and more numerous; brachycephalous, platyrhinian, woolly headed, and, when unaffected by the higher culture of the surrounding peoples, a pure forest dwelling savage." This photograph of an inmate of Bilibid Prison, Manila, is from the "Album of Philippine Types" by Daniel Folkmar, Manila, 1904. (Figure 12.)

types are to be found there. Among Europeans the other types resemble the Iberian; among negroes the other types resemble the Australoid; and among Orientals the other types resemble the Primitive.

May it be presumed that the Primitive and Iberian types conjoined in prehistoric times at some place in the East of Asia or nearby, and produced by fusion the Australoid. From this union innumerable offshoots have sprung in Southern Asia, the islands bordering that region, and in Africa. The Primitive type remains pure in parts of the East, and the Iberian type in Europe. If this hypothesis be untrue, the reverse of it should be considered; viz., the

Australoid forms the basic stock of all humanity, and the Iberian of Europe on one side and the Primitive of the Orient on the other are derivatives. If neither hypothesis be true, at least the Negrito of Mariveles has Primitive and Iberian characteristics ingrafted from without.

ANCIENT TYPES STILL EXIST.

The composition of any group of people, large or small, depends on the relative proportions of each type that entered into the composition of the group, the time during which the types have been in contact, the conditions of food, water, air, habits, etc., and other factors. There is no evidence that any type of man that ever existed has disappeared entirely, although there is evidence that the types have become somewhat modified in different parts of the world.

My recent discovery of paleolithic man in the Philippines (*Homo philippinensis*) may throw light on the origin of the Australoid type among the Negritos and among the inland tribes as well as in the littoral population of the archipelago. *Homo philippinensis* is Australoid in form yet somewhat different from the Negrito Australoid, the Igorot Australoid or the Australoid of the coast, but not so different as to preclude all relationship. *Homo philippinensis* is also related to the earliest forms of man in Europe: *Homo monsteriosis* and *Homo Heidelbergensis;* it is of the Neanderthal type, and is not greatly different from the Negro Australoid. The following hypothesis is plausible. Primordial man remained practically unaltered in form in Africa and the Pacific, and *Homo philippinensis* is this unchanged form; but in Africa the kinky hair and black skin of the negro may be added features due to environment, and the Australoid negro is a further modification of the negroid form, due also to environment. Whatever the cause may be, there are at present three types found among the Negritos—the Australoid, the Primitive and the Iberian; and these three types are found also among the other peoples of the Philippines wherever I have examined them. The Primitive and the Australoid, and doubtless the Iberian, are found in all the islands of the Pacific where search has been made and careful analysis of the people has followed, the first two types often among more or less pure Negritos.

The more profound the study, the more profound becomes the impression that the Primitive, the Australoid and the Iberian are the three fundamental types of mankind.

A Race of White Canaries

Establishment of a race of white canaries is described in a recent number of Knowledge, by Maud S. Martin. It started with a hen from a much in-bred buff strain, that sported to pure white, and was mated with an unrelated buff male. All of the offspring were buff, but when bred together gave 48 buff chicks and 18 white ones. Matings carried through the third generation seemed to prove that the color was subject to Mendelian laws, buff being dominant and white recessive. Twenty-five extracted recessives were secured and are now breeding pure in every generation. They seem to be fully as vigorous as the type, and have black, not pink, eyes.

A Eugenics Bibliography

The Russell Sage Foundation Library of New York City has published (as number three of its bulletin, February, 1914) a useful selected list of works on eugenics, from the material represented in its own collection. It is interesting to note that only 24 out of the 57 entries are due to Americans. Unfortunately, the list does not, for the most part, include the subjects of heredity and genetics, unless they deal solely with the eugenics phase.

THE REASON FOR EUGENICS

Religious Duty of Man to Assist the Course of Evolution and Endeavor to Make it Less Slow and Painful—His Power to do so Shown by the Results of His Half Unconscious Efforts in the Past.

FRANCIS GALTON.[1]

OUR ignorance of the goal and purport of human life, and the mistrust we are apt to feel of the guidance of the spiritual sense, on account of its proved readiness to accept illusions as realities, warn us against deductive theories of conduct. Putting these, then, at least for the moment, to one side, we find ourselves face to face with two great and indisputable facts that everywhere force themselves on the attention and compel consideration. The one is that the whole of the living world moves steadily and continuously towards the evolution of races that are progressively more and more adapted to their complicated mutual needs and to their external circumstances. The other is that the process of evolution has been hitherto carried out with, what we should reckon in our ways of carrying out projects, great waste of opportunity and life, and with little if any consideration for individual mischance. Measured by our criterion of intelligence and mercy, which consists in the achievement of result without waste of time or opportunity, without unnecessary pain, and with equitable allowance for pure mistake, the process of evolution on this earth, so far as we can judge, has been carried out neither with intelligence nor ruth, but entirely through the routine of various sequences, commonly called "laws," established or necessitated we know not how.

An incalculable amount of lower life has been certainly passed through before that human organization was attained, of which we and our generation are for the time the holders and transmitters. This is no mean heritage, and I think it should be considered as a sacred trust, for, together with man, intelligence of a sufficiently high order to produce great results appears, so far as we can infer from the varied records of the prehistoric past, to have first dawned upon the tenantry of the earth. Man has already shown his large power in the modifications he has made on the surface of the globe, and in the distribution of plants and animals. He has cleared such vast regions of forest that his work that way in North America alone, during the past half century, would be visible to an observer as far off as the moon. He has dug and drained; he has exterminated plants and animals that were mischievous to him; he has domesticated those that serve his purpose, and transplanted them to great distances from their native places. Now that this new animal Man finds himself somehow in existence, endowed with a little power and intelligence, he ought, I submit, to awake to a fuller knowledge of his relatively great position, and begin to assume a deliberate part in furthering the great work of evolution. He may infer the course it is bound to pursue from his observation of that which it has already followed, and he might devote his modicum of power, intelligence and kindly feeling to render its future progress less slow and painful. Man has already furthered evolution very considerably, half unconsciously, and for his own personal advantages, but he has not yet risen to the conviction that it his religious duty to do so deliberately and systematically.

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[1]These words of the founder of the science of eugenics are part of his chapter on "The Observed Order of Events" in *Inquiries into Human Faculty*, first published in 1883. The book is now obtainable at a cost of 35 cents, in "Everyman's Library", and should be in the possession of everyone who has even the slightest interest in eugenics.

"REVERSION" IN PRICKLY PEARS

Origin of All Spineless Varieties of Opuntia to be Sought in Long Selection From Spiny Ones—Plant May Produce Spiny Joints on One Side and Spineless Ones on the Other.[1]

DAVID GRIFFITHS,
Agriculturist, Office of Farm Management, Bureau of Plant Industry, U. S. Department of Agriculture, Washington, D. C.

IN THE economic handling of the prickly pear as a farm crop in southern Texas great differences have been found in the ease with which spines are burned from different species, and the differences are to a considerable extent matters of geographical position. Native species from the immediate vicinity of Brownsville have been employed in our economic plantings, because it was early recognized that these were the best adapted to the conditions and would make the largest tonnage of any that could be grown.

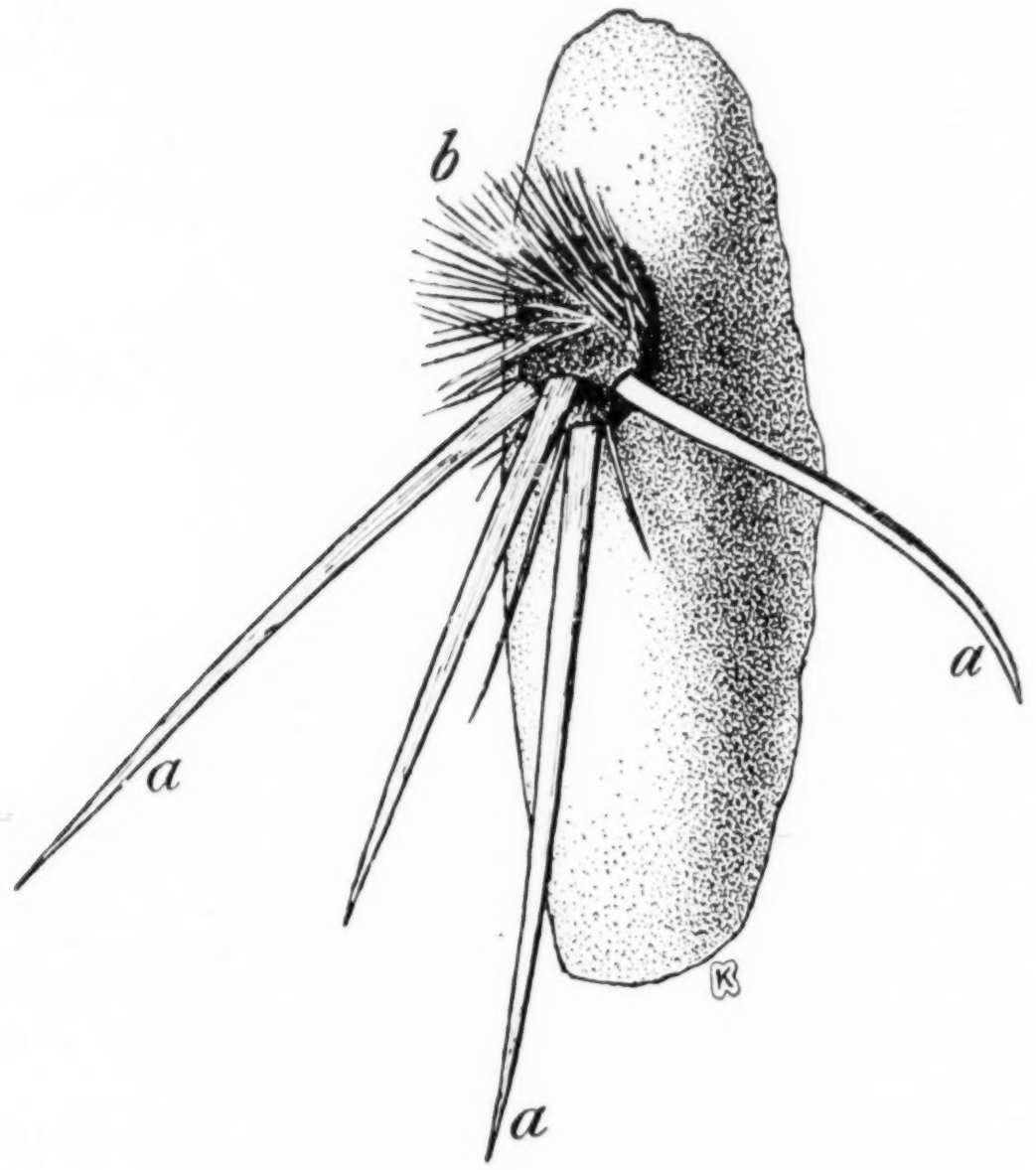
All species of this region are exceedingly spiny. They are even more spiny than those of the San Antonio region; moreover, the spines remain green for a longer period and are consequently much more difficult to singe properly preparatory to being fed. This is a serious economic consideration and one which is difficult to prevent. Under the humid conditions of the coastal region the tendency is for the spines (fig. 13, *a*) and the spicules (fig. 13, *b*) of all species to become dry much more slowly than in the more arid atmosphere of the regions farther inland.

In the cultivation of this crop it is therefore necessary for one to choose between the spineless forms not needing singeing and the much more productive spiny native varieties, which are not only difficult but often impossible to singe properly. It may be possible in time to breed varieties better adapted than the native ones, but the development of such forms from the spiny native prickly pears of the delta of the Rio Grande is an almost hopeless task, the variation in the number of spines produced being so trifling as to scarcely warrant selection, while they do not appear to hybridize readily with the spineless forms.

In June, 1905, the Office of Foreign Seed and Plant Introduction of this Bureau received from Dr. G. Borg, San Giovanni, Island of Malta, a few cuttings of a large spineless species, to which was assigned S. P. I. No. 14807. The plant passes in both this country and the Mediterranean region as spineless. About half a dozen cuttings of this importation were planted and grew for two and a half years at the plant introduction field station at Chico, Cal. They were then cut up into individual joints and re-established in nursery form in a plantation as extensive as the stock thus produced would permit. It was noticed that one of the original plants had on one of its joints three or four spines in one or two pulvini on one side. No heed was paid to this, however, and this joint was lost sight of in the planting, which was made in nursery form. In other particulars this did not differ from the other plants.

This prickly pear belongs to what commonly passes for *Opuntia ficus indica* and is nearly spineless. Frequently short spines are produced, but they are very few in number; there is but one in an areole, and they are usually only two to five or six millimeters in length. It is on the whole an average of the spineless forms. When

[1]This article is an extract from Mr. Griffith's Bulletin No. 31, U. S. D. A., "Behavior, under Cultural Conditions, of Species of Cactus Known as Opuntia," Washington, December 30, 1913.



SPINES AND SPICULES

A pulvinus, or cushion of spines (*a*) and spicules (*b*), from the edge of a joint of *Opuntia lindheimeri* from the San Antonio region of Texas. Ordinary "spineless cactus," although it may lack spines, often possesses spicules, which make it equally worthless for many of the purposes for which it is sold. It is only the most highly improved varieties that are absolutely free from spicules as well as spines, and hence suitable for feeding to live stock, without first being singed. (Fig. 13.)

the first planting of this stock was grown, one plant which was normally vigorous showed a few rather long spines, as stated above, similar in character to the shorter ones more commonly produced, but much longer and stouter.

The establishment of the nursery plantation took place in April, 1908. The following spring all of the spineless forms (especially S. P. I. No. 14807) were cut back to the original cutting for stock. At this time it was noticed that one side of one plant of this number was very spiny and the other side as spineless as the remainder of the importation. With the rest, this plant was cut loose, but the spiny cuttings were rejected. In the spring of 1910 the new growth was again in part spiny, and it was again cut back, but one joint was left attached to the original one. At that time neither joint left on the plant showed any spines.

In the spring of 1911 it was found that the previous season's growth had come mainly from the upper cutting, which grew in the season of 1909. From the original cutting two joints had sprung, one from each side. All new growth on one side of the plant, whether from the original cutting or from the younger joint, was practically spineless, while the other side was exceedingly spiny, the latter resembling the more common forms of some of the mission pears grown in all of the collections in southern California and bearing two to four white spines two-thirds of an inch to one and one-half inches long. The whole plant is shown in fig. 14.

The appearance of the plant in the spring of 1911 was so striking that it was pruned back but very little, simply enough to shape it up. Several of both spiny and spineless joints were planted, but owing to the condition of the

A CASE OF "DEMI-REVERSION"

This supposedly spineless Opuntia from the Island of Malta, when planted at Chico, California, began to develop spines—but only on a part of the plant. Sections taken from the spiny part produce spiny plants; cuttings from the spineless side produce plants which are spineless. Mr. Griffiths considers this to be a case of reversion which indicates that the original ancestor of the modern Opuntias was spiny. (Fig. 14.)

ground when the planting was done all made a very poor growth. However, the small amount of growth that was made came true to the characters of the individual cuttings planted. In the spring of 1912 the previous season's growth was again spiny on one side of the plant and spineless on the other, the contrast producing a very striking appearance.

This habit of reversion or bud variation, whichever it may be considered, is a very important characteristic, and, while more striking in the above variety than any other which has been cultivated in these investigations, appears to be not at all uncommon. A plant of another spineless variety on hand now bears promise to be just as conspicuous a few years hence as this one. It started to vary in the same direction last year.

All this relates, however, to the development of characters economically un-

desirable. It is well known that lack of spines is also a character which can be maintained by vegetative propagation, but being usually of slight variation is not as striking as the case mentioned of variation in an opposite direction. This phenomenon is an important one, for it furnishes a possible suggestion regarding the origin of the spineless varieties.

Both of these variations appear to the writer to point to the origin of the spineless species of the so-called *ficus-indica* group from the spiny ones, the spineless forms being the result of a long series of selection, both upon this continent and in the Mediterranean region, subsequent to their introduction there. The striking variation of certain spineless forms to a spiny condition is looked upon as a reversion to an original type.

Variation by Loss of Inhibitors

A hypothesis to explain variation by the appearance of new dominant characters as due to the loss of some single inhibiting factor, is put forward by William Bateson, director of the John Innes Horticultural Institution at Merton, Surrey, England, and the most distinguished exponent of Mendelism in England, in an address before the seventeenth International Congress of Medicine, reprinted in the Lancet.

"We have been accustomed," he remarks, "to speak of characters as added to or subtracted from the sum total of an individual composition, but as to the actual origin of new dominants there is very little contemporary evidence.

"In the case of those animals, such as the fowl, the wild original of which we think we know, many factors exist in our domesticated breeds which are not possessed by any of the wild species. No wild species, for example, has the complicated combs that are possessed by our domestic breeds. None has the dominant white color of the leghorn; and many other such instances might be given. How have these elements been added to the composition of the fowl?

" . . . but for one feature in the descent of dominants we might be tempted to suggest that they also arose through the introduction of some element from without into the system. I see no *a priori* impossibility in such a belief, but in these dominants we know that the distribution among the germ-cells is approximately symmetrical, and nothing that we know of animals or plants justifies the suggestion that a foreign element can be so treated in gameto-genesis. Perverse as such a suggestion may appear, I do not think we should close our minds to the possibility that these dominants arise by a process of some inhibiting factor. Until we have some far more direct method of recognizing the presence of factors this suggestion cannot be positively gainsaid."

The Importance of Genetics

Evolution, selection, heredity, environment, differential fertility, are not mere vague terms of biological science having no application to human life. On their right understanding depends the stability or collapse of human society in general and of our nation in particular.—Karl Pearson: Nature and Nurture (1910).

EFFECTS OF INBREEDING

A Valuable Instrument, But Fraught With Danger—Experiment Shows That if Long Continued, Its Effects are of Themselves Evil, Regardless of the Strength of Constitution of the Animals Involved.[1]

H. KRAEMER,

Professor in the Landwirtschaftliche Hochschule of Hohenheim, Germany.

DURING the past few months I have been receiving a continually increasing number of inquiries about the present status of inbreeding. People are nowadays constantly reading and hearing of its absolute harmlessness, while until a short time ago it was held up to them as utterly unpermissible. It seems desirable, then, to review some of the results of experience, and to state my own views as to whether consanguineous breeding really results injuriously.

The reliable observations made for many years past in every possible branch of breeding and in all kinds of herds, have thoroughly illuminated the question for anyone who is able to follow the truth when he sees it. But I must conclude, from the great number of letters I receive, that men are not able to follow the truth very often. And although the conclusions justified by these breeding experiments be known, yet some emphasis should be laid on the other side as well. The new and better knowledge of the great value of properly-directed inbreeding will surely not cause anyone to go astray. And certainly it is not for a minute my intention to lead anyone astray on this subject.

The present age has directed researches about consanguineous breeding in the right direction. If we are thereby led to indorse in all branches of stock-breeding a procedure so much feared in earlier times, that lies in the nature of things. Our delight in the discovery of the high importance of a principle always leads us too easily to pass by the darker sides, the aspects which should cause us to hesitate; and the serious results following inbreeding are now too lightly regarded in handbooks of stock-breeding. And when isolated breeders go so far as to declare that inbreeding produces no serious consequences whatever, then it is again time to recall the innumerable cases recorded from the experience of well-known breeders of the past. In this retrospect, the literature of the various civilized countries in earlier days furnishes a picture that can not be misinterpreted, and I shall therefore attempt to present it here. Such a literary review will perhaps be able to correct our viewpoint while broadening it, now that the English breeders have become such unconditional friends of inbreeding.

DARWIN'S INVESTIGATIONS.

Among the English writers who have dealt with the subject, Darwin naturally comes first to mind. We are too much accustomed to think of his work as going wholly backward and dealing only with origins. I want, therefore, at this point to commend most warmly to breeders the perusal of his book on "The Variation of Animals and Plants under Domestication." What a mass of stimuli to us in our work! What a mighty faculty for observation! What a surpassing talent for bringing innumerable details together to make a unified picture! This book should be in the possession of every breeder who wishes

[1]Translation of "Uber die ungünstigen Wirkungen naher Inzucht," in Mitteilung der Deutsches Landwirtschafts Gesellschaft, 6 and 13 September, 1913.

to deepen his knowledge of all branches of stock-breeding.

In the seventeenth chapter Darwin discusses "The Good Effects of Crossing and the Evil Effects of Close Interbreeding." He states his own observations and cites a long list of English authorities, both theoretical and practical. In following his outline of the case, I shall also cite some of the older French and German breeders.

Since the time of von Lehndorff, we have become more exact in defining the closeness of inbreeding. Darwin himself complains that a definition of close breeding is difficult because the same degree of consanguinity in different species of animals is reached in different manners, yet today we hold gladly to the idea of "free generations" and the "line of ancestors," and apply it to all kinds of animals. Such an accurate measure is highly desirable here, and according to it the pairing of father with daughter or of mother with son is the closest degree of consanguineous breeding. Not improperly, however, does Darwin himself note on this point that as Sebright has remarked, the pairing of brother and sister is really the closest degree of family mating, because here like blood is mated to like, while father in mating with daughter unites with only half his own blood. If evil results occur from inbreeding, they must, therefore, be the most serious when siblings or members of the same fraternity (full brother and sister) are paired.

But can any evil results be proved from inbreeding itself? "That any evil directly follows from the closest inbreeding has been denied by many persons," says Darwin, "but rarely by any practical breeder; and never, so far as I know, by anyone who has largely bred animals which propagate their kind quickly. Many physiologists attribute the evil exclusively to the combination and consequent increase of morbid tendencies common to both parents; and that this is an active source of mischief there can be no doubt. It is unfortunately too notorious that men and various domestic animals endowed with a wretched constitution, and with a strong hereditary disposition to disease, if not actually ill, are fully capable of procreating their kind. Close interbreeding, on the other hand, often induces sterility; and this indicates something quite distinct from the augmentation of morbid tendencies common to both parents. The evidence immediately to be given convinces me that it is a great law of nature, that all organic beings profit from an occasional cross with individuals not closely related to them in blood; and that, on the other hand, long-continued close interbreeding is injurious."

PROOFS NOT ALL CONVINCING.

When the proofs that Darwin gives, many of which have already found their way into our textbooks, are examined most carefully, it must be admitted that it is sometimes very difficult clearly to recognize the evils of inbreeding supposed to be shown by them. The results are often only too general in their nature, and admit of being strongly influenced by the constitutional soundness or unsoundness of the animals concerned. And finally, a considerable decrease in strength of constitution may be accompanied with so much of advantage to the breeder that he will completely overlook it in many instances. Whenever, therefore, the statements of breeders are subjective, they offer no proof, in the biological acceptation of the word; and often inbreeding may be credited with such and such results, when these results are due solely to breeding for high performance without regard to the constitution and vitality of the animals. As Darwin strikingly showed, an actual increase in vigor is clearly enough evident when cross-breeding is practiced. If this is undisputed, the conclusion ought to be well-established, that continuous line-breeding must finally call forth the opposite result.

Sebright, Knight, Youatt and Low have unanimously borne witness that continued close interbreeding is impossible. Prinsep also found that despite the best feeding, the growth of the animals steadily decreased, and John Sinclair expressly testifies that Bake-

well's method of breeding could not be carried on for any length of time with good results—although it was long in fashion because of Bakewell's great influence.

If Darwin did not appeal to these sources of evidence, it was not, of course, because they were not entitled to command respect. Besides, he refers only casually to Culley, although the latter is one of the most positive of the older English writers, on the absence of danger from inbreeding. The greatest hindrance to the improvement of breeds of live-stock, Culley declares, is due to the superstition that because of the danger of inbreeding, one should use the same bull only three years at the longest. Weak and delicate produce is feared, and many have fallen into the belief that inbreeding is abhorrent to religion. Happily, however, there are still people in England who have no regard for such superstitions, and the experience of many years justifies them. By the example of the half-wild cattle in Chillingham Park in Northumberlandshire, Culley seeks to demonstrate that even long-continued inbreeding is harmless. This race, the purest-bred in Europe, is hardy, sound and of good form, and has not changed in color for more than 500 years.

THE CHILLINGHAM CATTLE.

The Chillingham cattle are so often quoted in German literature that it seems worth while to give Darwin's remarks on the subject verbatim. Notice that according to his view even here inbreeding has not proved wholly innocuous:

"The half-wild cattle, which have been kept in British parks probably for 400 or 500 years, or even for a longer period, have been advanced by Culley and others as a case of long-continued interbreeding within the limits of the same herd without any consequent injury. With respect to the cattle at Chillingham, the late Lord Tankerville owned that they were bad breeders. The agent, Mr. Hardy, estimates (in a letter to me dated May, 1861) that in the herd of about 50 the average number annually slaughtered, killed by fighting, and dying, is about 10, or one in five. As the herd is kept up to nearly the same average number, the annual rate of increase must be likewise about one in five. The bulls, I may add, engage in furious battles, of which battles the present Lord Tankerville has given me a graphic description, so that there will always be rigorous selection of the most vigorous males. I procured in 1855 from Mr. D. Gardner, agent to the Duke of Hamilton, the following account of the wild cattle kept in the Duke's park in Lanarkshire, which is about 200 acres in extent. The number of cattle varies from 65 to 80; and the number annually killed (I presume by all causes) is from 8 to 10; so that the annual rate of increase can hardly be more than one in six. Now in South America, where the herds are half-wild, and therefore offer a nearly fair standard of comparison, according to Azara the natural increase of cattle on an estancia is from one-third to one-fourth of the total number, or one in between three and four, and this no doubt applies exclusively to adult animals fit for consumption. Hence the half-wild British cattle which have long been interbred within the limits of the same herd are relatively far less fertile. Although in an unenclosed country like Paraguay there must be some crossing between the different herds, yet even there the inhabitants believe that the occasional introduction of animals from distant localities is necessary to prevent 'degeneration in size and diminution in fertility.' The decrease in size from ancient times in the Chillingham and Hamilton cattle must have been prodigious, for Professor Rutimeyer has shown that they are almost certainly descended from the gigantic *Bos primigenius*. No doubt this decrease in size may be largely attributed to less favorable conditions of life; yet animals roaming over large parks, and fed during severe winters, can hardly be considered as placed under very unfavorable conditions."

GAME IN PRESERVES.

The question whether sustained close-interbreeding in game preserves leads to

degeneracy is also of great interest. One reads so much on this question without getting a distinct impression, that a real proof for the degenerative effects of inbreeding seems to be obscured by the very form in which it is presented. Darwin went into the question of deer at some length, and he finally comes to the general conclusion that continued interbreeding here, too, leads to harm.

"As some of our British parks are ancient," he says, "it occurred to me that there must have been long-continued close interbreeding with the fallow deer (*Cervus dama*) kept in them; but on inquiry I find that it is a common practice to infuse new blood by procuring bucks from other parks. Mr. Shirley, who has carefully studied the management of deer, admits that in some parks there has been no admixture of foreign blood from a time beyond the memory of man. But he concludes 'that in the end the constant breeding in-and-in is sure to tell to the disadvantage of the whole herd, though it may take a very long time to prove it; moreover, when we find, as is constantly the case, that the introduction of fresh blood has been of the very greatest use to deer, both by improving their size and appearance, and particularly by being of service in removing the taint of "rickback" if not of other diseases, to which deer are sometimes subject when the blood has not been changed, there can, I think, be no doubt but that a judicious cross with a good stock is of the greatest consequence, and is indeed essential, sooner or later, to the prosperity of every well-ordered park.'"

In referring to the longhorns of Bakewell, Darwin also takes the view adopted by Youatt. Continued inbreeding led in this case to a weakness of constitution not compatible with good management, and reproduction became uncertain. At the very commencement, however, Darwin naturally mentions the Shorthorns, among which the most extended use of inbreeding was notoriously made. So, says Darwin, "the famous bull Favourite (who was himself the offspring of a half-brother and sister from Foljambe) was matched with his own daughter, granddaughter and great-granddaughter, so that the produce of this last union, or the great-great-granddaughter, had 15-16ths or 93.75% of the blood of Favourite in her veins. This cow was matched with the bull Wellington, having 62.5% of Favourite blood in his veins, and produced Clarissa; Clarissa was matched with the bull Lancaster, having 68.75% of the same blood, and she yielded valuable offspring."

RESULTS WITH SHORTHORNS.

The numerous examples from Shorthorn herds show that consanguineous breeding of a very wide extent may be practiced without leading to the extinction of a herd. They do not show, however, that continuous inbreeding is in general uninjurious. Darwin himself well shows that even Collins practiced crossing. And while Bates, that successful breeder, practiced close inbreeding for 13 years, and had, as Darwin remarks, "the most exalted idea of the value of his herd," yet during the next 17 years he was obliged three times to introduce new blood into it. Bates himself said, according to Darwin's account, "that to breed in-and-in from a bad stock was ruin and devastation; yet that the practice may safely be followed within certain limits when the parents so related are descended from first-rate animals."

With sheep also, according to Darwin, inbreeding has been much practiced. Messrs. Brown are said never to have introduced new blood into their Leicesters during 50 years, and we have of course no right to question the truth of the breeders' statements in such instances. Mr. Barford, in the course of a half century, had come to the conclusion that inbreeding did no harm, if practiced on animals quite sound in constitution, but on the other hand he does not claim to have practiced breeding in the closest degrees of affinity. He adds, however, that most great breeders of sheep have protested against continued close interbreeding. Jonas Webb is said to have kept five separate families to work on, in order to retain the requisite distance of relationship between the sexes.

As concerns swine, Darwin also cites a long list of English witnesses, part of whom had written on the subject, and part of whom had given him their views orally. "With respect to pigs," Darwin says, "there is more unanimity amongst breeders on the evil effects of interbreeding than, perhaps, with any other large animal. Mr. Druce, a great and successful breeder of the Improved Oxfordshires (a crossed race), writes, 'without a change of boars of a different tribe, but of the same breed, constitution can not be preserved.' Mr. Fisher Hobbs, the raiser of the celebrated Improved Essex breed, divided his stock into three separate families, by which means he maintained the breed for more than 20 years, 'by judicious selection from the *three distinct families*.' Lord Western was the first importer of a Neapolitan boar and sow. 'From this pair he bred in-and-in, until the breed was in danger of becoming extinct, a sure result (as Mr. Sidney remarks) of inbreeding.' Lord Western then crossed his Neapolitan pigs with the old Essex and made the first great step toward the Improved Essex breed.

A REMARKABLE CASE.

"Here is a more interesting case. Mr. J. Wright, well-known as a breeder, crossed the same boar with the daughter, granddaughter, great-granddaughter, and so on for seven generations. The result was that in many instances the offspring failed to breed; in others they produced few that lived; and of the latter many were idiotic, without sense, even to suck, and when attempting to move could not walk straight. Now it deserves especial notice, that the two last sows produced by this long course of interbreeding were sent to other boars, and they bore several litters of healthy pigs. The best sow in external appearance produced during the whole seven generations was one in the last stage of descent; but the litter consisted of this one sow. She would not breed to her sire, yet bred at the first trial to a stranger in blood. So that, in Mr. Wright's case, long-continued and extremely close interbreeding did not affect the general form or merit of the young; but with many of them the general constitution and mental powers, and especially the reproductive functions, were seriously affected."

With regard to dogs an attempt has often been made to show that inbreeding has no influence, according to Darwin, Meynell's famous foxhounds, for example, being cited as having been bred closely in-and-in without the least trace of injury being discernible. J. Sebright, however, declares that he had seen "the offspring of strong spaniels degenerate into weak and diminutive lapdogs," as a result of the pairing of brother and sister. And W. D. Fox noted that with consanguineous breeding of bloodhounds a visible degeneracy took place, and that almost all the animals had an enlargement of the bones of the tail. A single crossing with foreign bloodhounds restored their fecundity and drove away the tendency to malformation in the tail. "Considering how rapid is the natural increase of the dog," Darwin reflects, "it is difficult to understand the large price of all highly improved breeds, which almost implies long-continued close interbreeding, except on the belief that this process lessens fertility and increases liability to distemper and other diseases."

DATA FROM FOWLS.

Of the greatest importance for the question of inbreeding are, finally, the authorities gathered by Darwin relating to the breeding of fowls. According to Sebright's statements his bantams became almost infertile through inbreeding, and the cocks, losing the secondary sexual characters, approached a female type; but the slightest degree of reversion to the proper male type—for example, the elongation of the two principal tail feathers by only half an inch, brought with it "an improved probability of increased fertility."

According to Wright's testimony Mr. Clark's fighting-cocks were so much affected by prolonged inbreeding that they allowed themselves to be knocked over without showing fight. Their weight decreased, to the great injury of their prize-winning power. Through a crossing with Mr. Leighton's stock,

they regained both weight and spirit. If the evils of inbreeding are overemphasized here, Ballance, the successful breeder of Malays, stands on another platform. Inbreeding does not necessarily involve the deterioration of the stock, he believes; "but all depends on how this is managed. My plan has been to keep about five or six distinct runs, and to rear about 200 or 300 chickens each year, and to select the best birds from each run for crossing. I thus secure sufficient crossing to prevent deterioration."

"With pigeons," says Darwin in conclusion, "breeders are unanimous that it is absolutely indispensable, notwithstanding the trouble and expense thus caused, occasionally to cross their much-prized birds with individuals of another strain, but belonging, of course, to the same variety. It deserves notice that, when size is one of the desired characters, as with pouters, the evil effects of close interbreeding are much sooner perceived than when small birds, such as short-faced tumblers, are valued. The extreme delicacy of the high fancy breeds, such as the tumblers and improved English carriers, is remarkable; they are liable to many diseases, and often die in the egg, or during the first moult; and their eggs have generally to be hatched under foster-mothers.

"Although these highly prized birds have invariably been subjected to much close interbreeding, yet their extreme delicacy of constitution can not perhaps be thus fully explained. Mr. Yarrell informed me that Sir J. Sebright continued closely interbreeding some owl-pigeons, until from their extreme sterility he as nearly as possible lost the whole family. Mr. Brent tried to raise a breed of trumpeters, by crossing a common pigeon, and recrossing the daughter, granddaughter, great-granddaughter and great-great-granddaughter with the same male trumpeter, until he obtained a bird with 15-16th of trumpeter's blood; but then the experiment failed; for 'breeding so close stopped reproduction'. The experienced Noumeister also asserts that the offspring from dove-cotes and various other breeds are 'generally very fertile and hardy birds'; so again, MM. Boitard and Corbié, after 45 years' experience, recommend persons to cross their breeds for amusement; for, if they fail to make interesting birds, they will succeed under an economical point of view, 'as it is found that mongrels are more fertile than pigeons of a pure race.'"

CONCLUSIONS WARRANTED.

I have here gone back to literal quotations from Darwin, because the views of this great naturalist, who was also an expert breeder, so exactly coincide with the general experience of practical men. I will freely admit that some of the evidence given in his examples is of damage done by inbreeding, and some of it merely of the results of pairing two animals which are not of first-class strength; the effects of bringing together two strains of weakness are thus mistaken for the effects of inbreeding. But if one admits that famous breeders thoroughly understand the necessity of breeding only from sound animals, then it must be admitted that these numerous examples prove that continued inbreeding *in itself* is injurious. As far as the practical production of farm animals goes, inbreeding does not offer itself as a desirable method; but this, however, is well understood. I might easily quote a great number of further instances from Darwin, which lead one to want to use caution in the practice of inbreeding. At the same time, however, I must say that in England the question has been illuminated just as it has for us, and it is absolutely wrong to believe, as we are so often told, that the English unconditionally advocate and practice inbreeding in their herds.

Things went the same way in France. If we examine Sanson, Baron, Magne, Cornevin and other important writers on the question of inbreeding, we find that here again views are decidedly diverse. Sanson[2] fully recognizes the potency of inbreeding, and the right places for its application, without sufficiently defining the different grades of close interbreeding. Baron takes a

[2]Sanson, Traité de Zootechnie, Paris, 1896.

slightly different view, pointing out that farmers finally get a glimpse of the fact that their animals are getting to be altogether too similar. Sexual differentiation gives place to a sort of neutrality, which may finally lead to sterility. Magne admits that it is very difficult to decide whether there is an actual power for evil in inbreeding as such, or whether any damage is wholly due to the union of two strains of weakness or the inheritance of a double dose of tendency to some disease. In practice, however, he advocates proceeding on the assumption that inbreeding does lead directly to injury. Cornevin alone, the most important of all, looks on inbreeding somewhat more leniently. And as he bases his views on a lot of first-hand observations, I will consider his views in a little more detail.

THE DEFINITIONS OF CORNEVIN.

Cornevin vigorously demands a distinction between the different grades of consanguinity. It makes a great deal of difference, he declares, whether one makes a "direct" pairing, "for example, father or grandfather, with daughter or granddaughter" or a "collateral" pairing (for example, uncle and niece). If inbreeding is held in so little esteem, it is because hasty generalization from single instances has been allowed to throw blame on all forms. The results of lateral pairing can not, in general, be compared with those of direct mating. Well, both now and in the past we have been accustomed to make the same distinction; only we were accustomed to speak more generally merely of "closer" or "wider" inbreeding; and today we have a measure for it, as has already been said, in the line of ancestors, or free generations. The plausibility of Cornevin's view can be proved by actual experimental breeding.

Next he alludes to the closeness and frequency of consanguineous marriages in antiquity, pointing out that these have not been wholly injurious. But against such a conclusion, drawn from human society, innumerable examples may be quoted, particularly where incestuous marriages were considered allowable. The Veddas of Ceylon, among whom marriage between brother and sister is said to be the rule, are really incapable of civilization, and also physically degenerate. As for the ancient Greeks, it is true that they permitted consanguineous marriages of very close degree, but we have no evidence as to the frequency of such unions with them. But after all: the results of close inbreeding often show themselves only after it has been continued a long time. Experiments in human society can certainly be quoted to throw light on the question of inbreeding, but not in the sense Cornevin understands.

HIS EXPERIMENTS.

Cornevin made limited experiments in inbreeding his own cattle, sheep and swine very closely. His Hollanders were inbred for 12, his Jerseys for seven years in this manner, without showing any evil results, and the prizes which the animals won each year at district shows certainly indicate that they did not present evidence of degeneracy. The *Merinos Châtillonais* had been inbred for 11 years at the time that Cornevin wrote his *Traité de Zootechnie générale* (1891) and were as little degenerated as the cattle. On the other hand, this ardent defender of inbreeding admits that it can not be practised with Yorkshire and Essex swine for more than two or three generations. These animals have such an inclination to lay on fat, that it leads to sterility and lack of milk on the part of the sows, when their tendency is accentuated by line-breeding. Indeed, adds Cornevin, the mere necessity of keeping them in a pen and feeding them highly became harmful, and it is questionable whether the result would not have been very different under more favorable circumstances.

Pigeons regularly multiply adelphogamically—that is, brother mates with sister; and so do swans. geese, guinea-fowl and ducks very often, according to Cornevin. He pursued inbreeding for 11 years with pigeons and geese, and there was no perceptible change of color, weight or fecundity. Guinea-fowls also showed no change, but Houdans and Crevecoeurs suddenly developed a strong

tendency to albinism. From experiments with rabbits it was decided that solid-colored animals with golden-brown hair, showing occasional spots of white, began to turn entirely white after the fourth generation. Cornevin therefore suddenly jumped to the conclusion that the effects of inbreeding differed with different animals, and that one could not dogmatically assert, for all species, that it was wholly harmless. He does not specifically state whether he was selecting the strongest animals to breed from, but his indorsement of inbreeding gives us good reason to believe that he was anxious, in his experiments, to keep the two factors carefully separated. He wanted to observe the results of inbreeding in itself—*par elle même*—and not to throw additional light on the well-known fact that an increased tendency to degeneracy may take place through the union of two tendencies to weakness and blemishes, when consanguineous matings are made.

MANY DIFFERENCES OF OPINION.

It is, accordingly, a mistake to think that only among us Germans is there such a diversity of opinion on the subject of inbreeding, and it is equally exaggeration to think that it is we alone who are so doctrinaire in these matters. In all civilized countries, the views on inbreeding and hybridization used to be based on the haps and mishaps of experiments and actual breeding. We have indeed carried on the controversy about pedigree-breeding and individual potency with the greatest keenness, and in the side-issues of this controversy inbreeding of course occupies a prominent place. But the heart of the question, whether inbreeding, in and by itself, results in evil, is warmly debated not only among us, but everywhere else. If our old theories about blooded stock were one-sided on that point, if they did not sufficiently distinguish between pure breeding and inbreeding, we can also reproach the individual-potency theory nowadays with very often being too general and disregarding the various aspects of the question, when it argues its conception of "crossing."

Buffon had published precepts that were—speaking as a breeder—simply crazy, but he did not lack followers. Hartmann, for example, supports him. "Never," he says, "must one give to a stallion mares of the same breed, nor must he permit the mating of two animals from the same stud. It cannot be too strongly recommended that one should endeavor as much as possible to cross his breeds of horses." Such views won the day, and they created a real danger. When experienced breeders, such as Wollstein, Ammon, Justinus, Mentzel, von Weckherlin, and others took a unanimous stand in sharp opposition, they performed a great service. But the too wide application of their own doctrines had a doubtful side. Though one might apply their principles, in an extensive practice of inbreeding, and secure satisfactory results, yet the laws underlying the whole question were not properly made clear, and the theorizers on pedigree-breeding did not even then get a full view of certain items which we now think must be considered in the technical application of the principles of inbreeding, if success is to be had or failure to be avoided. He who started in to practice the principles they laid down, often had to pay the consequences. And although von Weckherlin, thinking that the prejudice against inbreeding arose solely as the result of experience with sheep, questions whether these consequences are appreciable, the resultant careless practice of inbreeding was unfortunate.

IT WORKS BOTH WAYS.

Computations can indeed be made, that the practice led to fineness of wool, but the disease—the actual failure of many breeders—can also be traced very easily to the same practice. The widespread creation of definite herd-types through line-breeding has led to many damaging results in the sheep industry, and on general princip.es we ought to anticipate in all other branches of live stock breeding, that the building up of herds characterized by certain very definite traits would have a tendency to limit variation and thereby to destroy the prerequisite of evolutionary progress. Such a feeling has naturally

existed among breeders, so that from hoary antiquity to our own days it has always been recognized that one should attempt inbreeding only with the very best stock. Hermann von Nathusius has publicly expressed himself in a masterly manner "On Inbreeding or Pairing in Close Relationship."[3] As a reaction against much of the one-sided pedigree talk, his declarations had their place as an expression of the period, just as Hermann von Nathusius himself was a child of the period. Today he would express himself very differently on many points could he familiarize himself with the evidence as to the dangerous possibilites of inbreeding which has been acquired by many of the younger experimenters; but he is, unfortunately, no longer with us. All the more must it be recognized how objectively he expressed himself in his clear, simple and logical way, on the main point of our thesis, the evil results of inbreeding. As the Thoroughbred advocates dared to assume without qualification the position that aversion to consanguineous mating was not founded on natural considerations, but derived from moral ones and bolstered up by a false deduction from the evil effects of pairing diseased and blemished animals—then it was high time to state the facts in the case, in the form of general principles. For criticising these extremists and for pointing out the actual status of the question, Hermann von Nathusius is highly to be commended. But we of today must take with a grain of salt the ideas that he put in the form of five propositions, to end his discussion of the case:

1. The mating of closely related animals can result in good progeny; some of the best improved breeds have been built up by that method.

2. Up to date no indisputable example has been given of close family-breeding carried on among animals for even 30 years.

3. Among many of the most fortunate and exemplary breeds, in-and-in breeding has resulted in deviations from the standard.

4. Consanguineous breeding alone, therefore, can not be laid down as an adequate method of breeding.

5. On the other hand, it would be equally undesirable to abandon it bodily.

Had Hermann von Nathusius known the studies going on as to the origin of prominent breeds and herds, he would have qualified his first statement to say that not only some, but most, of the best breeds of improved live stock resulted from line-breeding. But indeed, he has expressed this idea more strongly elsewhere. And his last two propositions he would undoubtedly change, to the disadvantage of close interbreeding. For today we have learned through the studies of von Lehndorf, von Oettingen, de Chapeaurouge, Rau and younger writers, how important it is that a herd should have a widely-spreading pedigree. But I will not place this side of the question in the foreground just at this time.

Pusch, too, who was unhappily taken from us so prematurely, endeavored to show the dangers of inbreeding experimentally. More and more I am convinced that all trustworthy and properly controlled experiments in this direction lead us to the same conclusion: *Continued inbreeding always must result in weakened constitution, through its own influence.* The more we realize today that high transmissible performance seems to be bound to individual animals, and the more we endeavor to make use of the blood of these individuals through inbreeding, so much the more must we anxiously guard against deterioration of strength and shape. Otherwise we shall fall into the error made by the early enthusiasts for pedigree breeding, who by taking an unduly narrow view spoiled an idea which in and by itself was sound. Knowledge and experience will guide our great breeders in the right way. If, however, the idea that inbreeding is never harmful begins little by little, and in ever wider circles, to spread, then the evils which it involves must once more be adequately pointed out.

[3]Kleine Schriften und Fragmente über Viehzucht, Berlin, 1880.